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A  
JOURNAL  
OF  
NATURAL PHILOSOPHY,  
CHEMISTRY,  
AND  
THE ARTS.

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VOL. XI.

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Illustrated with Engravings.

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BY WILLIAM NICHOLSON.

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BY WILLIAM D. HOWARD

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## PREFACE.

**T**HE Authors of Original Papers in the present Volume, are Mr. Thomas Reid; Mr. James Scott; Mr. Boswell; Mr. John Gough; Mr. Irvine; Mr. Cuthbertson; Mr. William Henry; John Bostock, M. D.; W. Brande, Esq.; Mr. Matthew Murray; Mr. J. C. Hornblower; Mr. A. F. Thoelden; Mr. John Clennell; J. P.; W. N.; W. F. S.; Mr. William Wilson; A. Thomson, Esq.; Amicus; Mr. O. Gregory; Mr. Ezekiel Walker; Mr. James Stodart; Count Rumford, V. P. R. S.; Mr. Thomas Harrison. A Constant Reader.

Of Foreign Works, Mr. Erman; M. Bonnard; Messrs. Robertson and Sacharoff; Lalande; A. M.; Constat Duméril; C. L.; A. B. Berthollet; J. C. Delametherie; M. Bralle; Professer Pini; Mr. Deyeux; M. Hassenfratz; Mr. Goettling; Mr. Steinacher; Mr. Marechaux; Mr. Schnaubert; Mr. J. G. Schmidt; Brugnatelli; Citizen Duhamel; C. L. Cadet; J. Machlachlan; Humbold and Bonpland.

And of English Memoirs abridged or extracted; Charles Hatchett, Esq. F. R. S.; Mr. C. Waistell; Mr. George Dodd; Dr. William Roxburgh; Captain Joseph Brodie; Mr. William Hardy; Mr. James Rawlinson; Humphry Davy, Esq. F. R. S.; Richard Chenevix, Esq. F. R. S. M. R. I. A.; Mr. Robert Seppings; A. Carlisle, Esq. F. R. S.; John Churchman, Esq. Imp. Acad. of Sciences, Petersburg; Mr. D. Mushett, Sir A. N. Edelcrantz; Edward Bigott, Esq.

## PREFACE.

Of the Engravings, the Subjects are, 1. A very simple and perfectly safe Gun Lock, by Mr. Dodd. 2. The framing of a Field Gate with a considerable Accession of Strength, by Mr. Waistell. 3. A Compensation Curb for a Time Piece, by Mr. Scott. 5, 6. Plans and Sections of the Bavarian Salt Works. 7. Captain Brodie's Method of connecting Iron Bars, and coating them with Lead. 8. Mr. Rawlinson's Mill for grinding Colours. 9. Apparatus for grinding Indigo and other dry Materials. 10. Method of banking the Balance of a Time Piece, by Mr. Hardy. 11. A Pyrometer for measuring the Heat of a Furnace, by Mr. J. G. F. Schmidt. 12. A Portable Steam Engine, by Mr. Murray. 13. Apparatus for making the Gaseous Oxide of Carbon, by Mr. Baruel. 14. A Topographical Plan by Mr. Churchman, in which the Elevations and Depressions of the Surface of the Earth, are by a new Method correctly delineated. 15. Mr. Gregory's Apparatus for measuring the Power of Horses. 16. The American Borer. 17. Diagram illustrative of Optics, by Mr. Ezekiel Walker. 18. Mr. Sepping's Blocks for supporting and suspending Ships. 19. Count Rumford's Apparatus for illustrating his Experiments in Proof, that the greatest Density of Water is higher than the freezing Point. 20. A Valve for Steam Engines, by the Chevalier Edelcrantz. 21. Figures and Diagrams, by Edward Pigott, Esq. to illustrate the Causes of Periodical Decrease in Diminution of Light, in a Star in Sobieski's Shield. 22. A Press for Botanical Specimens, by Mr. Harrison.



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THE ARTS.

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MAY, 1805.

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ARTICLE I.

*Letter from Mr. THOMAS REID, on the Construction of Time-keeping Machines.*

To Mr. NICHOLSON.

SIR,

IN your interesting and useful Journal of December last, I was glad to see the improvement of compensation pendulums for astronomical clocks, so zealously taken up by such an able hand, as that of Mr. Edward Troughton's.

Compensation  
pendulum of  
Mr. Edward  
Troughton.

But an excellent clock of this sort becomes so very valuable and necessary an appendage in an observatory, to those astronomical instruments with which he is so happily engaged in daily constructing and improving; that he must more readily see their advantage than even those whose business it is to make such clocks. If Mr. Berthoud, a celebrated author on every part that regards the improvement of time-keeping machines, is correct, it would appear, that the steel wires of Mr. Troughton's pendulum must be too slender. Mr. Berthoud, by his experiments, saw, that there was a certain strength of materials necessary, in order to render the compensation complete having found, that on the pendulum rods (if too small) being lengthened by heat, the contraction by cold would not

Probability from  
Berthoud's ex-  
periments that  
the steel wires  
may be too slight.

bring the ball again quite up to the place where it set out from when the heat was first applied, and this with a ball of a moderate weight; no doubt the weight of the ball may be made subservient to any size of wires.

Mr. Troughton seems to have attended to this.

Whether Mr. Troughton has attended to these circumstances, I know not, but suspect from his proposing, yet, to make some sort of pyrometrical apparatus for the further proving of his pendulums, that the complete compensation has not been fully ascertained, only in so far as regards the calculation of the relative expansions of brass and steel.

The improvement of clocks, and time-keeping machines of every description, more particularly those destined for astronomical purposes, is a subject that has not a little engaged my time and attention.

The pendulum of Ludlam with a wooden rod examined.

There is a pendulum, having a wooden rod, the construction of which is described with great neatness, perspicuity, and mechanical knowledge, by its author, (Mr. Ludlam, late an eminent professor at Cambridge) in his essays, and recommended by him, *who was no mean judge*, as one of the best in almost every respect, particularly in so far as regards the impulse from the clock taking place through the middle line or centre of the rod, to be thence communicated in the same line to that of the ball, that hence no circumgyratory motion should take place. Now, although the principle set out with *here*, seems completely adapted to prevent this sort of motion, it will be found on trial, as I did, that of all pendulums yet made, it is the most liable to generate this very sort of motion.

It is liable to a side oscillation of the ball round the rod as an axis.

The ball being the middle frustum of a globe, a form whose matter is much spread out from the centre to the edge, and having a large hole, for the rod to pass through; *this* taking away much of the matter from the centre, tends much more than the lenticular form, to produce the motion Mr. Ludlam wished to avoid. Another great fault of this pendulum, is, that of putting in screws through the wooden rod, to clip the flat part from the crutch; now in changes of weather from moist to dry, or by heat and cold, these screws will accordingly be found, sometimes to pinch the flat part of the crutch, and at other times, to leave it at liberty, or even to allow it to have a considerable degree of shake between the screw points: hence will arise very different degrees of impulse communicated to the pendulum ball. Wood has a very sensible la-

The screws which are acted on by the crutch do not preserve a constant distance,

titudinary

titudinary alteration, by the effects of heat and cold, or by dry and moist, yet these effects on its length are, rather but very imperceptible, or at least, are, in so small a degree, as have not been well ascertained, to what extent they are; even by those who have made experiments with it on the pyrometer.

Mr. Berthoud condemns wood as being unfit for pendulum rods, and although he, and others have given tables of the expansion of various materials, yet none of them have condescended to say, what were the effects of heat and cold on wood of any sort.

I am well convinced, that a pendulum may be so fitted up with a wooden rod, as to perform with such a degree of correctness, that it would be a very difficult matter to say, whether it, or the best compensation pendulum yet constructed, when both comparatively tried, was the nearest to accurate measuring of time.

There has been one circumstance attending all those pendulums fitted up with a wooden rod, that their errors have been imputed to the rod, when in fact, they ought to have been imputed to that of the ball, and these errors have arisen from the manner by which the ball is hung on the rod, resting on its lower edge on the regulating nut; and lead having a considerable degree of expansion, clocks having such pendulums have been found, by those who attended properly to their going, to have gone constantly faster in summer than in winter. Let the ball therefore be hung by its centre on the rod, and a much greater degree of accuracy in time-keeping will be seen to follow.

In consequence of my trials with Mr. Ludlam's pendulums, they were found to be extremely troublesome to put on beat, from their strong tendency to this gyratory sort of motion, it being some while, before they would come to move steadily; I hit not only on a method of putting a clock, as it were mechanically on beat, (the common way being by the ear) but was led to think on a way of constructing a pendulum, in which this gyratory motion could hardly take place, even although the pendulum should be but indifferently fitted up. This last was by following a method quite the reverse to that of Mr. Ludlam's, in making the pendulum ball, which I made in the usual or lenticular form, and in order that it should have as much of its matter preserved at the centre, there were two



steel wires put through the ball, passing parallel to each other, and each put a little to one side of the centre, through which pendulum rods are usually made to pass, and when the rod is wood, it necessarily takes away much of the matter from the centre of the ball.

A pendulum of this sort of mine, and which has a degree of compensation in it, I made to a clock, which my brother got, and which he has at his house, No. 31, Rosamond Street, Clerkenwell.

The experiment applied to watches, &c.

I mentioned this mode of putting clocks mechanically on beat, to my ingenious friend, Mr. Pennington, who has since very happily applied the same successfully in his practice to watches, &c.

Pendulum for regulating the striking part of clocks; not new.

I see you have mentioned in your Journal of July last, the application of a pendulum to regulate the striking part of clocks, from the Society for encouraging Arts, &c. having given a premium for it to Mr. Maffy.

This is not a new thing. Mr. Berthoud mentions it as his invention, and you will see a drawing, and the description of it, in his *Essai sur l'Horlogerie*, published in 1763. Julien Le Roy, in my humble opinion, is certainly intitled to the merit of it, as it appears to me, that Berthoud has taken the idea of it from Le Roy's method of regulating the striking train of his repeating watches, which he *invented*, and applied to them about the year 1754.

The crank escapement, not new;

There was a premium given also in 1799, by the honourable society above mentioned, for a new 'scapement by Mr. Goodrich\*; now this 'scapement was made prior to the year 1740, and invented by the Abbé Soumille; and another nearly of the same sort was made at Rome before that period, as may be seen in the collection published by Thiout, in the year 1741. Surely nobody would think of adopting such a 'scapement as this, whose principle seems to be that of depriving the pendulum of the most valuable property it possesses, viz. that of having the liberty to operate freely under the influence of gravitation. This 'scapement keeping the pendulum, as it were in leading strings.

and bad.

I am surprised that none of the members of this honourable and useful society, should not have known, that these

† See Philos. Journal, quarto series, III. 342, 416. It is a crank.—N.

things for which premiums have been adjudged were not new, perhaps they were nevertheless *jd*, to those to whom the premiums were adjudged.

Among Thiout's collection may be seen a 'scapement, which <sup>A 'scapement made long ago</sup> he gives to the ingenious Dutertre, about the year 1724. Peter Le Roy gave into the Royal Academy of Arts and Sciences in the year 1727, a description of the same 'scapement which Dutertre claimed, or pretended to say was his invention. The mistake lays with Thiout, for Dutertre's 'scapement is an improvement of a very old one, used by the Germans in large clocks, perhaps long before the year 1600, though neither the author of it, nor the time *when*, can now be traced.

However it was before Dr. Hook's time, who invented one of the same kind before 1658. I would not have entered so minutely into this discussion, but to show the progress of the duplex, which in its present form, was first made so by Peter Le Roy, who afterwards gave it up for a bad one; yet it ought <sup>leading to the duplex 'scapement which was first made by Peter Le Roy.</sup> to be allowed that that of Dutertre's must have led him very easily to it. So much for the duplex 'scapement, so called by the workmen, and now in such general use. A celebrated Philosopher in the supplement to the Edinburgh Encyclopedia, under the article watch work, has given it a French surname, that of Dupleix, for what reason, I know not.

Thiout has given also among the number, a 'scapement of <sup>The origin of the common detached 'scapement ascribed to Thiout.</sup> his own, a sort of detached one, and which may be considered as the foundation of the detached one of the present day, now so much improved, and of such general use in all our pocket and box chronometers: indeed it seems a scapement indispensably necessary for these purposes. Yet Berthoud in his famous time-keeper, No. 8, used a very different one from that of the detached sort.

I am,

Sir, with much esteem, Your's,

THOMAS REID.

Edinburgh, 25th March, 1805.

## II.

*An Analysis of the Magnetical Pyrites; with Remarks on some of the other Sulphurets of Iron. By CHARLES HATCHETT, Esq. F. R. S. From the Philosophical Transactions for 1804.*

(Concluded from page 276.)

## § VII.

Whether the artificial pyrites with minimum of sulphur be, like the natural, magnetical.

SO far, therefore, as can be proved by similarity in chemical properties and analysis, the magnetical pyrites is indisputably a natural sulphuret, completely the same with that which till now has been only known as an artificial product; but, that the mind may be perfectly satisfied, another question must be solved, namely, how far do they accord in receiving and retaining the property of magnetism? common pyrites do not appear to affect the magnetic needle, or, if some of them slightly act by attraction, (which however I never could perceive, nor recollect to have read in works expressly relating to magnetism,) yet they do not possess, nor appear capable of acquiring, any magnetic polarity. As, therefore, the iron of pyrites is undoubtedly in the metallic state, and in a considerable proportion, the destruction of this characteristic property of metallic iron, must be ascribed to the other ingredient, sulphur.

The artificial compound is not a mere mixture.

But we have lately seen, that a natural combination of iron with 36.50 or 37 per cent. of sulphur, is in possession of all the properties supposed hitherto to appertain (in any marked degree) almost exclusively to the well known magnetic iron ore; and that the combination alluded to is strictly chemical, and not (as at first might have been imagined) a mixture of particles of magnetic iron ore with common pyrites\*.

The compound directly formed at red heat.

This is certainly very remarkable; and it induced me to examine the effects produced by sulphur, on the capacity of metallic iron for receiving and retaining the magnetic properties. I therefore prepared some sulphuret of iron, by adding a large quantity of sulphur to fine iron wire, in a moderate red heat.

\* This has been sufficiently proved, by the facts which have been stated; I shall however add, that upon digesting a mixture of the powder of common pyrites and iron filings in muriatic acid, I only obtained hydrogen gas, exactly as if I had employed the iron filings without the pyrites.

The



The internal colour and lustre of the product, were not very unlike those of the magnetical pyrites; and, after the mass had been placed during a few hours between magnetical bars, I found that it possessed so strong a degree of polarity, as to attract or repel the needle completely round upon its pivot; and although several weeks have elapsed since it has been removed from the magnetical bars, it still retains its power, with little diminution; like the magnetical pyrites, however, in its natural state, it is not sufficiently powerful to attract and take up iron filings.

But this sulphuret did not contain so much sulphur as the magnetical pyrites; I therefore mixed some of it, reduced to powder, with a large quantity of sulphur, and subjected it to distillation in a retort, which was at length heated until the intire bulb became red.

The sulphuret, by this operation, had assumed very much the appearance of the powder of common pyrites, in respect to colour; but, in its chemical properties, such as solubility in muriatic acid, with the production of sulphuretted hydrogen gas, as well as in the nature of the precipitates it afforded with prussiate of potash and with ammonia, it perfectly resembled the magnetical pyrites. Moreover, by analysis, it was found to consist of 35 parts of sulphur and 65 of iron; and although (being in a pulverulent state) its power, as to receiving and retaining the magnetic property, could not so easily be examined, yet, by being powerfully attracted by the magnet, with some other circumstances, there was every reason to conclude, that in this respect also it was not inferior.

Another proportion of sulphuret was formed, as above described; it was placed between magnetical bars, and, in like manner, received and retained the magnetic power.

It is certain, therefore, that when a quantity of sulphur equal to 35 or 37 per cent. is combined with iron, it not only does not prevent the iron from receiving the magnetic fluid, but enables it to retain it, so that the mass acts in every respect as a permanent magnet.

Black oxide of iron, by one operation, does not appear to combine with sulphur so readily as iron filings; a second operation, however, converts it into a sulphuret, very much resembling that which has just been described, including the chemical

resembled common pyrites; but was capable of magnetism.

It contained less sulphur than the magnetical pyrites. More sulphur added and low ignition.

It was attractable by the magnet.

Hence 36 or 37 per cent. does not prevent iron from becoming a magnet.

Black oxide of iron combines with sulphur so readily.

chemical as well as the magnetical properties; but, undoubtedly, by these processes, it is progressively converted, perfectly or very nearly, into the metallic state.

Iron combined with a larger proportion of oxygen, such as the fine gray specular iron from Sweden, will not form a sulphuret by the direct application of sulphur, in one operation; although it becomes of a dark brown colour, partly iridescent, and is moderately attracted by a magnet.

Magnetical  
pyrites combined  
with 9 per cent.  
more of sulphur,

Fifty grains of the magnetical pyrites, reduced to powder, and mixed with three times the weight of sulphur, were distilled in a retort, until the bulb became moderately red-hot. After the distillation, the pyrites weighed 54.50; consequently, the addition of sulphur was 9 per cent. making the total = 45.50 or 46 per cent. The powder was become greenish-yellow, very like that of the common pyrites: it did not afford any sulphuretted hydrogen, when digested in muriatic acid; but it nevertheless was partially dissolved, and the solution, when examined by prussiate of potash, and by ammonia, was not different from that of the crude magnetical pyrites.

—was still at-  
tracted by the  
magnet.

The powder which had been distilled with sulphur, and which had thus received an addition of 9 per cent. to its original quantity, *was still capable of being completely taken up by a magnet.*

Iron ceases to be  
acted on by the  
magnet when the  
dose of sulphur  
is at some point  
between 46 and  
52 per cent.

From the whole of the experiments which have been related, it is therefore evident, that iron, when combined with a considerable proportion of sulphur, is not only still capable of receiving the magnetic property, but is also thereby enabled to retain it, and thus (as I have already remarked) becomes a complete magnet; and it is not a little curious, that iron combined (as above stated) with 45 or 46 per cent. of sulphur, is capable of being taken up by a magnet, whilst iron combined with 52 per cent. or more, of sulphur, (although likewise in the metallic state,) does not sensibly affect the magnetic needle; and hence, small as the difference may appear, there is reason to conclude, that the capacity of iron for magnetic action is destroyed by a certain proportion of sulphur, the effects of which, although little if at all sensible at 46 per cent. are yet nearly or quite absolute, in this destruction of magnetic influence, before it amounts to 52. But, what the exact intermediate proportion of sulphur may be, which is adequate to produce this effect, I have not as yet determined by actual experiment.



As carbon acts on soft iron, (which, although it most readily receives the magnetic influence, is unable to retain it so as to become a magnet, without the addition of a certain proportion of carbon, by which it is rendered hard and brittle, or, in other words, is converted into steel, so, in like manner, does sulphur seem to act; for it has been proved, by the preceding experiments, that the brittle mass formed by the union of a certain proportion of this substance with iron, whether by nature or by art, becomes capable of retaining the magnetic virtue, and of acting as a complete magnet.

Sulphur acts on iron like carbon in causing it to contain magnetism,

This remarkable coincidence, in the effects produced on iron by carbon and sulphur, induced me to try the effects of phosphorus; and my hope of success was increased by the remark of Mr. Pelletier, who says, that "the phosphuret of iron is attracted by the magnet;" \* and therefore, although certain bodies may be thus attracted, without being capable of actually becoming permanent magnets, I was desirous to examine what might be the power, in this respect, of phosphuret of iron.

—and so likewise does phosphorus.

I therefore prepared a quantity of phosphuret of iron, in the direct way, viz. by adding phosphorus, cut into small pieces, to fine iron wire made moderately red-hot in a crucible. The usual phenomena took place, such as the brilliant white flame, and the rapid melting of the iron, which, when cold, was white, with a striated grain, extremely brittle, hard, and completely converted into a phosphuret. The fragments of this were powerfully attracted by a magnet; and, after I had placed two or three of the largest pieces, during a few hours, between magnetical bars, I had the pleasure to find that these had become powerful magnets, which not only attracted or repelled the needle completely round, but were able to take up iron filings, and small pieces, about half an inch in length, of fine harpsichord wire; and, although they have now been removed from the magnetical bars more than three weeks, I cannot discover any diminution of the power which had thus been communicated to them.

Experiment. Phosphuret of iron was made;

—and by the touch formed a powerful magnet.

The three inflammable substances, *carbon*, *sulphur*, and *phosphorus*, which, by their chemical effects on iron, in many respects resemble each other, have now therefore been proved

\* "Le Phosphure de Fer est attirable a l'aimant." *Annales de Chimie*, Tome XIII. p. 114.

alike to possess the property of enabling iron to retain the power of magnetism; but I shall consider this more fully in the following section.

## § VIII.

From the whole which has been stated we find,

General results.  
Magnetic pyrites  
is a British  
mineral,

—containing  
about 37 sulphur  
and 63 iron.

It differs in its  
properties from  
common pyrites  
which contains  
more sulphur.

It is identically  
the same as the  
artificial sul-  
phuret.

In common py-  
rites the sulphur  
predominates,

1. That the substance called magnetical pyrites, which has hitherto been found only in Saxony and a few other places, is also a British mineral, and that, in Caernarvonshire, it forms a vein of considerable extent, breadth, and depth.

2. That the component ingredients of it are sulphur and metallic iron; the former being in the proportion of 36.50 or 37, and the latter about 63.50 or 63.

3. That the chemical and other properties of this substance are very different from those of the common martial pyrites, which however are also composed of sulphur and iron, varying in proportion, from 52.15 to 54.34 of sulphur, and from 47.85 to 45.66 of metallic iron; the difference between the common pyrites which were examined being therefore 2.19, and the mean proportions amounting to 53.24 of sulphur, and 46.75 of iron; consequently, the difference between the relative proportions, in the composition of the magnetical pyrites and of the common pyrites, is nearly 16.74, or 16.24.

4. That, as the magnetical pyrites agrees in analytical results, as well as in all chemical and other properties, with that sulphuret of iron which hitherto has been only known as an artificial product, there is no doubt but that it is identically the same; and we may conclude, that its proportions are most probably subjected to a certain law, (as Mr. Proust has observed in the case of the artificial sulphuret,) which law, under certain circumstances, and especially during the natural formation of this substance in the humid way, may be supposed to act in an almost invariable manner.

5. That, in the formation of common martial pyrites, there is a deviation from this law, and that sulphur becomes the predominant ingredient, which is variable in quantity, but which, by the present experiments, has not been found to exceed 54.34 per cent. a proportion, however, that possibly may be surpassed in other pyrites, which have not as yet been chemically examined,

6. That

6. That iron, when combined naturally or artificially with 36.50 or 37 of sulphur, is not only still capable of receiving the magnetic fluid, but is also rendered capable of retaining it, so as to become in every respect a permanent magnet; and the same may, in a great measure, be inferred respecting iron which has been artificially combined with 45.50 *per cent.* of sulphur.

Limits of the sulphur in magnetic pyrites.

7. That beyond this proportion of 45.50 or 46 *per cent.* of sulphur, (in the natural common pyrites,) all susceptibility of the magnetic influence appears to be destroyed; and, although the precise proportion which is capable of producing this effect, has not yet been determined by actual experiment, it is certain that the limits are between 45.50 and 52.15; unless some unknown alteration has taken place in the state of the sulphur, or of the iron in the common martial pyrites.

Proportion beyond which the magnetic influence is lost.

8. That, as carbon, when combined in a certain proportion with iron, (forming steel,) enables it to become a permanent magnet, and as a certain proportion of sulphur communicates the same quality to iron, so also were found to be the effects of phosphorus; for the phosphuret of iron, in this respect, was by much the most powerful, at least when considered comparatively with sulphuret of iron.

As carbon renders iron tenacious of magnetism, so also do sulphur and phosphorus.

9. And lastly, that as carbon, sulphur, and phosphorus, produce, by their union with iron, many chemical effects of much similarity, so do each of them, when combined with that metal in certain proportions, not only permit it to receive, but also give it the peculiar power of retaining, the magnetical properties; and thus, henceforth, in addition to that carburet of iron called steel, certain sulphurets and phosphurets of iron may be regarded as bodies peculiarly susceptible of strong magnetical impregnation.

Conclusion.

Having thus, for the greater perspicuity, reduced the principal facts of this Paper into a concise order, I shall now make some general observations.

It is undoubtedly not a little singular, that a substance like the magnetical pyrites, which, although not common, has been long known to mineralogists, should not hitherto have been chemically examined, especially as mineralogical authors have mentioned the analysis of it as a desideratum. The result of this which I have attempted, proves that it is really deserving of notice; for thus we have ascertained, that the sulphuret of iron hitherto known only as an artificial product, is also formed

Remarks.

The magnetical pyrites is an interesting product.

by



by nature; and that the composition of this last, agrees with those proportions of the artificial sulphuret which have been stated by Mr. Proust.

No intermediate  
natural product  
between the  
common and the  
mag. pyrites.

But, from this sulphuret or magnetical pyrites, I have not, by analysis, as yet been able to discover any regular or immediate gradations into the common pyrites; for the least proportion of sulphur in these amounted to 52.15, and the greatest proportion to 54.34; so that, between the magnetical and the common pyrites, the difference is considerable, in the proportions of their component substances, as well as in their physical and chemical properties; whilst the difference which I have hitherto been able to detect in the proportions of some of the common pyrites (very dissimilar in figure, lustre, colour, and hardness,) has only amounted to 2.19.

Remarks on  
Proust's experi-  
ments.

Mr. Proust, in a general way, considers common pyrites to differ from the first sulphuret, or that composed of 60 parts of sulphur and 100 of iron, ( $= 37.50$  per cent.) by containing a farther addition of half the above quantity of sulphur, or 90 parts of sulphur and 100 of iron, ( $= 47.36$  per cent.) but this opinion he appears to have formed, in consequence of results obtained by synthetical experiments made in the dry way.— Now, when we consider how difficult it is to regulate the high degrees of temperature, and what a numerous chain of alterations in the relative order of affinities most commonly result from alterations in these degrees of heat, it seems to me that we cannot rely, with absolute certainty, on synthetical experiments made in the above way, unless they are corrected, and contrasted with analytical experiments made on the same substances. But it does not appear, from the two memoirs published by Mr. Proust, to which I have so frequently alluded, that that gentleman did more, in respect to analysis, than distil the cubic and dodecaedral pyrites found near Soria, from which he obtained about 20 per cent. of sulphur; and, having observed that the residuum possessed the properties of the sulphuret which has been commonly prepared in laboratories, he concluded that the sulphur obtained from the pyrites, is the excess of that proportion which is requisite to form the sulphuret, the proportions of which, therefore, he by synthesis ascertained to be, as I have above stated,  $= 37.50$  of sulphur, and 62.50 of iron, or 60 of sulphur combined with 100 of iron; and lastly, having formed 318 grains of this sulphuret from 200 grains of iron



iron filings, he distilled the sulphuret with an additional quantity of sulphur, in an inferior degree of heat, and obtained 378 grains of a substance which, excepting density, was similar to the common martial pyrites.\*

It is however to be regretted, that Mr. Proust did not make a regular analysis of the pyrites of Soria, and of the residuum after distillation; for (unless these pyrites are very different from those which I have examined) he would most probably have found the proportion of sulphur greater than that which he has assigned to natural pyrites in general. This at least there is great reason to suppose, if we allow that most or all of the pyrites have been formed in the humid way, by which, we may conceive, a larger proportion of sulphur may be introduced into the compound, than can take place in high degrees of temperature. And this opinion is corroborated by the results of my analyses; for, instead of finding the general proportions to be 47.36 of sulphur and 52.64 of iron, the mean result of these analyses is very nearly the reverse, being 53.24 of sulphur and 46.76 of iron.

Mr. Proust is also of opinion, that the pyrites which contain the smallest quantity of sulphur, are those which are most liable to vitriolization; and, on the contrary, that those which contain the largest proportion, are the least affected by the air or weather.† This opinion of the learned professor, by no means accords with such observations as I have been able to make; for the cubic, dodecaedral, and other regularly crystallized pyrites, are liable to oxidizement, so as to become what are called hepatic iron ores, but not to vitriolization; whilst the radiated pyrites (at least those of this country) are by much the most subject to the latter effect; and therefore, as the results of the preceding analyses show that the crystallized pyrites contain less sulphur than the radiated pyrites, I might be induced to adopt the contrary opinion. But I am inclined to attribute the effect of vitriolization observed in some of the pyrites, not so much to the proportion, as to the state of the sulphur in the compound; for I much suspect, that a predisposition to vitriolization, in these pyrites, is produced by a small portion of oxygen being previously combined with a part, or with the general mass,

He did not make a regular analysis.

Proust apprehends that pyrites holding the least sulphur are most liable to vitriolization:

But this disposition most probably arises from a commencement of oxidation.

\* *Journal de Physique*, Tome LIV. p. 92.

† *Journal de Physique*, Tome LIII. p. 91.

of the sulphur, at the time of the original formation of these substances, so that the state of the sulphur is tending to that of oxide, and thus the accession of a farther addition of oxygen becomes facilitated. We have an example of similar effects in phosphorus, when (as is commonly said) it is half burned, for the purpose of preparing the phosphorus bottles; and the propensity to vitriolization, observed in many of the half-roasted sulphureous ores, appears to me to arise from this cause, rather than from the mere diminution of the original proportion of sulphur, or the actual immediate conversion of part of it into sulphuric acid; nevertheless, I offer this opinion, at present, only as a probable conjecture, which may be investigated by future experiments and observations.

The magnetical properties of the sulphuret is a remarkable fact,

The magnetical properties of the sulphuret of iron which forms the principal subject of this Paper, must be regarded as a remarkable fact; for I have not found, in the various publications on magnetism which I have had the means of consulting, even the most remote hint, that iron when combined with sulphur, is possessed of the power of receiving and retaining the magnetic fluid; and, judging by the properties of common pyrites, we might have supposed that sulphur annihilated this power in iron, as indeed seems to have been the opinion of mineralogists, who have never enumerated magnetical attraction amongst the physical properties of those bodies; and, although Werner, Widenmann, Emmerling, and Brochant, have arranged the magnetical pyrites with the sulphurets of iron, yet the magnetical property could not with certainty be stated as inherent in the sulphuret, for, at that time, this substance had not been subjected to a regular chemical analysis, and the magnetical property might therefore be suspected to arise from interspersed particles of the common magnetical iron ore. This probably has been the opinion of the Abbé Haüy; for, in his extensive Treatise on Mineralogy lately published, I cannot find any mention made of the magnetical pyrites, either amongst the sulphurets or amongst the other ores of iron.

little or scarcely noticed.

The magnet said to consist of iron with 10 to 20 oxygen.

In the mineral kingdom, a great variety of substances, and even some of the gems, exert a feeble degree of attraction on the magnetic needle, and sometimes also acquire a slight degree of polarity; \* but, as this wonderful property has only been

Cavallo on Magnetism, page 73.

observed

observed conspicuously powerful in one species of iron ore, this has been always emphatically called *the Magnet*,\* and is said to consist of metallic iron combined with from 10 to 20 per cent. of oxygen.

From the facts, however, which have been recently stated, The sulphuret is a different kind of magnet. we now find that there is another natural substance, apparently very different from the magnet in chemical composition, but nevertheless approaching very nearly to it in power, which is found in several parts of our globe, and particularly in a province of this kingdom, where it constitutes a vein, running north and south, of a considerable extent, and several yards in width and thickness.

From the experiments also, which have been made on the The phosphuret magnet. artificial preparation of this substance, we find, that it is capable of receiving the magnetic properties when the proportion of sulphur amounts to 37 per cent. and is still powerfully attracted when a much larger quantity of sulphur is present. There is, however, some point at which all these effects cease, and this point appears to be, when the sulphur is in some proportion between 45 or 46 and 52 per cent. The preceding experiments have also proved, that iron when combined with phosphorus, likewise possesses the power of becoming a magnet to a very remarkable degree; and, by the similarity, in this respect, of the carburet of iron called steel, to the above sulphuret and phosphuret, a very remarkable analogy is established between the effects produced on iron, by carbon, sulphur, and phosphorus.

Carbon, when combined in a very large proportion with iron, Of carburets, plumbago is not magnetical: other steel compounds form powerful magnets. forms the carburet of that metal called plumbago; a brittle substance, insoluble in muriatic acid, and destitute of magnetic properties. But, smaller proportions of carbon, with the same metal, constitute the various carburets included between black cast iron and soft cast steel;† bodies which are more or less

\* In a future Paper, it is my intention to give an account of some comparative analyses of the varieties of this substance.

† "When the carbon exceeds, the compound is carburet of iron or plumbago: when the iron exceeds, the compound is steel, or cast iron, in various states, according to the proportion. All these compounds may be considered as subcarburets of iron."—Thomson's System of Chemistry, Vol. I. p. 165.

Mr.



less brittle, soluble in muriatic acid, and more or less susceptible of magnetical impregnation; some of them form the most powerful magnets hitherto discovered.

Sulphur and iron have similar habits.

Sulphur, in like manner, combines with iron in a large proportion, forming the common pyrites, which are brittle, almost or quite insoluble in muriatic acid, and devoid of magnetical properties. Sulphur in smaller proportions, forms sulphurets which are also brittle, but are soluble in muriatic acid, and strongly susceptible of magnetical impregnation.

So likewise phosphorus and iron.

Phosphorus also, when combined with iron, makes it brittle, and enables it powerfully to receive and retain the magnetical properties; so that, considering the great similarity which prevails in other respects, it may not seem rash to conclude, that phosphorus (like carbon and sulphur,) when combined with iron in a very large proportion, may form a substance incapable of becoming magnetical, although, in smaller proportion, (as we have seen,) it constitutes compounds which are not only capable of receiving, but also of retaining, the magnetical properties, even so far as, in some cases, to seem likely to form magnets of great power; and, speaking generally of the carburets, sulphurets, and phosphurets of iron, I have no doubt but that, by accurate experiments, we shall find that a certain proportion of the ingredients of each, constitutes a maximum in the magnetical power of these three bodies. When this maximum has been ascertained, it would be proper to compare the relative magnetical power of steel (which hitherto has alone been em-

Mr. Musket, in the following Table, exhibits the proportion of charcoal which disappeared, during the conversion of iron to the different varieties of subcarburet known in commerce.

" Charcoal absorbed.			Result.
$\frac{1}{120}$	-	-	Soft cast steel.
$\frac{1}{160}$	-	-	Common cast steel.
$\frac{1}{80}$	-	-	The same, but harder.
$\frac{1}{30}$	-	-	The same, too hard for drawing.
$\frac{1}{25}$	-	-	White cast iron.
$\frac{1}{20}$	-	-	Mottled cast iron.
$\frac{1}{15}$	-	-	Black cast iron.

" When the carbon amounts to about  $\frac{1}{60}$  of the whole mass, the hardness is at the maximum." Thomson, Vol. I. p. 166; and Phil. Magazine, Vol. XIII. pp. 142 and 143.

ployed



ployed to form artificial magnets) with that of sulphuret and phosphuret of iron; each being first examined in the form of a single mass or bar of equal weight, and afterwards in the state of compound magnets, formed like the large horse-shoe magnets, by the separate arrangement of an equal number of bars of the same substance in a box of brass.

The effects of the above compound magnets should then be tried against others, composed of bars of the three different substances, various in number, and in the mode of arrangement; and, lastly, it would be interesting to make a series of experiments on chemical compounds, formed by uniting different proportions of carbon, sulphur, and phosphorus, with one and the same mass of iron. These quadruple compounds, which, according to the modern chemical nomenclature, may be called carburo-sulphuro phosphurets, or phosphuro-sulphuro-carburets, &c. of iron, are as yet unknown as to their chemical properties, and may also, by the investigation of their magnetical properties, afford some curious results. At any rate, an unexplored field of extensive research appears to be opened, which possibly may furnish important additions to the history of magnetism, a branch of science which of late years has been but little augmented, and which, amidst the present rapid progress of human knowledge, remains immersed in considerable obscurity.

An extensive field for experiments of interest and importance.

### III.

*Extract of a Memoir of Mr. ERMAN, entitled Observations and Doubts concerning Atmospheric Electricity.*

(Concluded from p. 300, Vol. X.)

AN electrometer furnished with a rod three feet in length, and placed in the open air, does not shew any divergence; but when the bent point of another electrometer, which also exhibits no divergence, is moved above the first, and even when the motion is parallel to the horizon, the leaves of the latter will be seen to diverge negatively, without the second giving any sign of electricity.

Two electrometers which do not indicate divergence alone, shew it when one is passed above the other.

It is very probable that the effect of these vapourous and aqueous meteorological masses is manifested at the points of the

This effect probably arises from the division of the electricity.

electrometer, by this same action, which depends on the division of the electricity, and this explains the sudden changes which supervene in the positive or negative electric state of the earth: it is not even probable that the clouds possess in themselves a negative electricity, or that the vapours of water should be always in this state. Mr. Erman thinks he can prove that the vapours of water are only negative when the body from which they proceed is insulated; but that they become positive as soon as it is brought into contact with the earth. Rain, after its fall, leaves bodies in the state of *minus* electricity, which is agreeable to the preceding experiments; snow produces this effect so much the better, as it leaves the surrounding air in a state of dryness, which augments its insulating properties.

The positive or negative state of aqueous vapour depends on insulation.

Influence of the clouds on the earth.

The clouds which have a tendency to rain or snow must necessarily produce their influence on the ground, and it is for this reason that the opposition of the cloud and the ground is so quickly manifested. It would be interesting to explain the complication arising from the variations which the electricity of the earth undergoes, as well with respect to its species as to its intensity, and Mr. Erman is at present engaged in this subject.

The different phenomena of the divergence of an insulated point explained by the same law.

With respect to the variable degree of the positive divergence of an insulated point fixed in the earth, it may perhaps be attributed to the greater or less conducting quality of the surrounding air: the point, when it is very well insulated from the earth, shows a certain degree of positive charge when the air is insulated so that electricity cannot be communicated to it; it shows zero when the conducting power of the air is nearly equal to the rapidity with which the charge is made; and negative when this power is very strong. These phenomena are thus explained by the same law.

Probability that there is no free electricity in the atmosphere.

Mr. Erman has also supposed in his memoir that there is not any quantity of electricity disengaged in the atmosphere; but he does not however assert that this is strictly the case: he has only sought to draw the attention of philosophers to this subject, and to show that these phenomena are equally well explained by the sphere of activity of the electricity.

Convenience of the instruments.

The experiments are very easily made, and the electrometers he uses are very portable; for when the leaves of gold are placed on one side of the cylinder, motion cannot occasion any accident. With respect to the sticks, they may be made of several

several pieces, and be screwed together when they are wanted. Such were the instruments Mr. Erman took with him over several hundred leagues; the stick was three lines in diameter at one end, and one line at the other. This philosopher does not yet know what may be the influence of the different thickness of the conductor. One of these conductors happened to be of brass, the other of steel; it did not appear to him that the difference of the metal occasioned any in the results. But he purposes making new enquiries on this subject.

Mr. Erman offers his conclusions with great diffidence: he does not pretend to have formed a new theory, but only to state his doubts on the opinion of those philosophers who have attributed the phenomena hitherto observed to a disengaged electricity in the atmosphere. He notices several other interesting experiments which he is employed in making upon smoke, and particularly on electricity in a vacuum. We shall hasten to publish the results of them as soon as they come to hand.

## IV.

*Description of a Compensation Curb.* By Mr. JAMES SCOTT.

To Mr. NICHOLSON.

SIR,

INCLOSED I send you a sketch of an instrument I have constructed, much wanted in the profession of watchmaking, for the purpose of publishing in your Philosophical Journal, which I presume will be of much benefit to the public, and at the same time may prevent any other person from claiming it. Introductory letter.

I shall call it a compensation curb. The construction of this instrument is for the purpose of expanding and contracting in the different temperatures, so as to counteract the error which the pendulum-spring is liable to by the smallest variation of heat or cold.

The inventions that have hitherto been put in practice for the same purpose, I beg leave to make a few remarks on.—The compound balance, when carefully made and adjusted, is certainly a very complete counteracting expansion, and will answer exceedingly well on board a ship, if there be no material difference in the density of the air; if otherwise, the balance



lance being loaded, will have to encounter a considerable deal more friction, and consequently be impeded in its vibrations: if worn in the pocket, it is also liable to error, as exercise will alter its diameter. The compound balance has heretofore got the preference, because artists have not been able to invent a compensation curb adjustable to the exact expansion required (which, by many experiments, I have proved the inclosed to be fully competent to); therefore, the plain balance having no projections on the surface, must certainly have the advantage. As I am not in the habit of expressing my ideas to the public, I hope you will have the goodness to rectify any errors in the stile, and also curtail any part which may appear unnecessary to you.

I remain, Sir,

Your much obliged humble servant,

JAMES SCOTT.

39, Grafton Street, Dublin.

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*The following is a Description of the Compensation Curb.*

Description of  
a compensation  
curb.

The steel index, *Pl. II. Fig. 2*, letter A, is for the purpose of supporting the curb, which is fastened by a screw and steady pin at R, the circle of which at A is turned with a dovetail, as shewn at H, and is slit so as to snap into the frame-plate, by which means it may be turned, and will carry the curb, so as to regulate the machine in the common way. D and E are two circles composed of brass and steel soldered together, the outside of E brass and the inside steel; but the outside of D is steel and the inside brass; so that the one circle expands when the other contracts; by which means the acting part of the curb at C will shift towards the index with heat, and prevent the vibrations being slower, which the expansion of P, the regulating spring and the balance, would otherwise occasion; and on the contrary with cold, it will shift its position nearer the stud I, which the regulating spring is pinned to; so that let it receive heat or cold, the acting part of the curb at C will at all times keep the regulating spring the exact length, to counteract the expansion of the balance and pendulum-spring. V is a piece of steel, with a notch cut in it to receive the expansion circles D and E. F is a screw for fastening V at any part of the circles; so that, by stoving the watch,

you

you will immediately ascertain towards what part of the circles you must shift V. If you find its rate flow with heat, you must shift V to lengthen your circles, and if fast, the contrary; and by marking your circles each time you have occasion to shift V, you will be enabled to adjust the curb to the exact expansion required. W is a loose piece of steel, which fits in the notch of V between the two expansion circles, to keep them fast in their proper positions when screwed by F. SS are two steady pins made fast in the frame-plate, which receive the circle E between them to prevent the action of P, the regulating spring, from affecting C, the curb, by moving it to or from the center during the going of the watch. The two expansion circles require to be made very delicate; it is therefore to be observed by the manufacturer of this instrument, that the brass in each circle is to be the thickness of the steel; so that when the two bodies are soldered together, they will make two thicknesses of the edge of a main-spring of a watch. The larger your watch will admit the diameter of the curb, the better. It must be at least the size of the balance.

Description of a  
compensation  
curb.

## V.

*Letter from Mr. BOSWELL, in Answer to AN OLD CORRESPONDENT.*

To Mr. NICHOLSON.

SIR,

April 10, 1805.

THE person signing *your Old Correspondent*, has thrown some very undeserved reflections on my last communication in your Journal.

Mr. Boswell  
vindicates him-  
self from confi-  
dence.

In answer to his remarks, I beg leave to observe that the introduction to my paper contains sufficient to justify me fully from his imputations.

I have there asserted no more than that "I have discovered a method of coming so near the truth, that should it turn out to be in reality not so exact as appears to me, yet it promises to be so useful for common computations that I am induced to send it for publication, if you approve of it."

I know

I know not, Sir, how I could have written in any way farther from "announcing my discovery with confidence" than the above, or indeed with more diffidence.

His methods  
were stated as  
nearly approach-  
ing the truth.

In the first place, I state it only as a *near approach* to the exact truth, which both your and his criticisms have fully proved it to be: In the next place, I express my doubt of its being even *so exact* as it appears to me: And thirdly, I have left its publication to depend *on your approbation*.

If this is not enough to remove all suspicion of confidence, I have to add, that the sentence which concludes that subject in the paper, states, that an unperceived error might arise from the smallness of the circles which I used; and besides this you can testify for me, that I wished the paper to be suppressed altogether before publication, when you shewed me that it even wanted an hundredth part of being exact, though with a much greater inaccuracy the matter contained in it would be useful for the purposes to which I stated it might be applied.

As to the proof of the second *fact*, it was not from the "conviction of its obvious accuracy not requiring proof" that I did not insert any, but because I concluded it must be sufficiently obvious after what I had stated of the first, that it was the same sort of *experimental* proof I had used for both: And if any gentleman will try the experiment as I did, he will find I have not misstated the matter.

There may indeed be some little impropriety in using the word *fact* in a popular sense, in any thing like a mathematical statement; but to notice such a trifle with inverted commas, only appears to indicate a spirit of cavilling on the part of your Old Correspondent.

None of the  
quadratures of  
the circle are  
more than ap-  
proximations.

But with all his precision he has forgot one fact, that puts his computations more on a level with my experiments, which is, that no method has ever yet been discovered of computing with *perfect exactness* the relative proportions of the circumference to the diameter, and of course to the other lines he mentions; and that it is only a far-laboured *approximation* to the truth that has been inserted in the work from whence he has extracted the proportions which he has used; and that therefore what he has "announced with so much confidence," (to use his own words) is not precisely demonstrative truth, but only an approach to it.

I did



I did not ever intend to recommend my method as a perfect and infallible geometrical problem applicable to the more sublime mathematical speculations, but as a more ready, and, permit me to add, more exact way than that in general use, for the humbler purposes of common life, such as measuring round timber, conduit pipes, engine cylinders, &c.

I am, &c.

J. WHITLEY BOSWELL.

## VI.

*Description of an improved Gate for Fields.* By Mr. CHARLES WAISTELL\*.

DEAR SIR,

THE various methods used in bracing common gates for fields; prove that not one of them is greatly superior to the rest; for, if it was, that method would have been generally adopted. Most gates are loaded with superfluous timber in some of their parts, and are constructed upon such bad principles, that they are frequently broken by their own weight, aided by the concussion of the head against the falling-post; and this, long before any part of the wood has begun to decay. I have for some time given this subject considerable attention, being impressed with the idea, that if common gates could be constructed with less timber, and upon better principles, the saving of timber only would be of national importance; for we have many millions of gates to uphold in Britain, and their numbers are annually increasing. The result of my labours has been the plan which accompanies this letter. Gates made according to it, possess great strength, are very light, and of easy and simple construction. Although uniformity of appearance be not essential in a common gate, yet is worth having when it can be obtained, as in this gate, without additional expense.

My gate is made with short, and consequently less valuable, oak or ash timber, than those of the commonest construction; its strength is much greater than any other gate made with a like

Mr. B.'s methods have practical utility.

Imperfections of common gates.

Great saving in timber if they were made less cumbersome.

New plan.

Account of the author's improvements.

\* Communicated in a letter to Charles Taylor, Esq. Secretary of the Society of Arts, who returned their thanks for the same. Vol. XXII. 1804.

quantity

quantity of timber, there being at four distant points between the head and the heel, two bars and a brace crossing each other: and I doubt not that it will be found proportionably more durable: it is, besides, very easy to construct, and requires less labour than most other common gates. Twenty-nine years ago I designed plans for ornamental gates, with semi-oval and semi-circular braces, and had them executed; the plans were sent to my friends in various distant parts of this kingdom, as also to Ireland; and I have the pleasure to observe, that they are become almost the only ornamental gate in many parts of England. The plans of them I never published, although they were prepared for engraving fifteen years ago; and I should be as indifferent about my present design, of a common field gate, if I did not conceive that its publication would materially benefit the public; the introduction of this form being, I conceive, of some national importance, as timber has been lately greatly enhanced in price, and is rapidly on the advance.

This gate was designed for the approach to a country residence; but for common purposes, the wicket on one hand, and the short length of rails on the other, may be omitted. I shall thank you, if you will have the goodness to lay my plan before your respectable Society, of which I have, for many years, had the honour to be a member. And should this plan be approved of, I may probably furnish some designs for park gates on an improved construction.

I am, Dear Sir,

Your very humble Servant,

CHARLES WAISTELL.

March 22, 1803.

Mr. Charles Taylor.

*Reference to the Engraving of Mr. Waistell's Gate.*

DIMENSIONS.—(Plate II. Fig. 1.)

Description and  
dimensions of  
the new con-  
structed field-  
gate.

The heel of the gate to be about	$3\frac{1}{2}$ inches square.
The head of ditto	$2\frac{1}{2}$ by 3 inches.
The top rail or bar	$3\frac{1}{2}$ by $1\frac{1}{2}$ inches.
The bottom bar	$3\frac{1}{2}$ by $1\frac{1}{4}$ inches.
The bar in the middle of the gate	3 by $1\frac{1}{4}$ inches.
The other bars, and the 4 braces	$2\frac{1}{2}$ by $1\frac{1}{4}$ inches.

*Observations*

*Observations on its Construction.*

The head and heel of the gate may be of oak, and the bars and braces of fir. Narrow and thick bars, when braced as in this design, are stronger than broad and thin ones, containing the same quantity of timber, and they also oppose a less surface to the wind. The two points in the heel of the gate, to which the thimbles are fastened, may be considered as firm or fixed points. From these points, viz. 1 and 2, two braces to proceed to 4 and 3, in the middle of the bottom and top bars, and being there secured, these become fixed points, and from these two points, viz. 4 and 3, two braces proceed to 5 and 6, fixing those points. The gate is thus doubly braced, viz. from the top of the heel to the top of the head, by means of the braces 1, 4, and 4, 5; and from the bottom of the heel to the bottom of the head, by means of the braces 2, 3, and 3, 6. On each side of the gate are two braces, and those parallel to each other. The brace proceeding from the bottom of the heel of the gate, and that which is parallel to it, as also the bottom bar, are all strained in the way of compression, and the brace proceeding from the top of the heel, and the other brace which is parallel to it, and also the top bar, are all strained in the way of extension. The strains in this gate being none of them tranverse, but all longitudinal, it would support a vast weight at its head without having its form altered. The braces all serve the double purpose of keeping the gate in its true form, and of shortening the bearings of the bars, and strengthening them. Few gates have less timber in their braces; and perhaps in no other way can a gate be so firmly braced with so small a quantity of timber.

At 5, 4, 7, and 8, two braces and a bar of the gate are firmly screwed together by means of iron pins and screw nuts. At the other points, where only one brace crosses a bar, common gate-nails are used.

If, in some cases, a strong top-bar be wanted, to resist the pressure of heavy cattle, a bar or board, about six inches broad, and one inch thick, may be laid with its broad side upon the top bar, and fixed thereto by means of the ends of the braces in the middle, and by the heel and head of the gate at the two ends of it. This board will, in this position, resist exactly the same



same pressure as a thick top bar, three inches broad, by four inches deep, although it contain no more than half the timber.

In the ground plan, or horizontal section, *Fig. 7.* represents a piece of wood, about four inches cube, pinned to the falling post, a little below the catch, to stop the gate from swinging beyond the post: another stop near the ground may be useful.

When gates are hung to open one way only, their heels and heads generally rest against the hanging and falling posts; but when they are hung according to this design, gates may be made about one foot shorter for the same opening, and consequently they must be lighter, stronger, and less expensive.

#### *Of the hanging of Gates.*

Hanging of  
gates.

When the two hooks in the hanging-post are placed in the same perpendicular line, a gate, like a door, will rest in any direction in which it may be placed. But, in order that a gate may shut itself when thrown open, the hooks are not placed exactly perpendicular; the upper hook declining a little towards the falling-post, or a few feet beyond it. In whatever direction that hook declines the farthest, in the same direction will the gate rest, if unobstructed, and its head cannot then sink any lower. Make the head describe half a circle, and it will thus have attained its utmost elevation, and will be equally inclined to descend either to the right or to the left\*.

Particular de-  
scription of the  
method of hang-  
ing a gate.

The following method of fixing the hooks and thimbles, will, I think, be found to answer very well for a gate that is intended to open only one way. Supposing the face of the hanging-post to be set perpendicular, and the upper hook driven in near its inner angle, as is represented in the preceding design, and that the lower hook must be four feet and a half below it; suspend a plumb-line from the upper hook, and at four feet and a half mark the post; then at one inch and a half farther from the gateway than this mark, drive in the lower hook; this hook must project about half an inch farther from the face of the post than the upper hook. In the section or ground-plan of the

\* See Chap. II. of Mr. Parker's Essay on the Hanging of Gates; and also the Agricultural Report for Northumberland, by Messrs. Bailey and Culley.

gate, the two white circles near the hanging-post represent the places of the two hooks when brought to the same horizontal line; that nearest the gateway represents the place of the upper hook. A line drawn through the middle of these two circles, and extended each way, will, on one hand, represent the gate's natural line of rest, and, on the other, the line of its highest elevation. A gate thus hung will, when thrown open nearly to the line of its highest elevation, return to the falling-post with a velocity sufficient to resist a moderately strong wind. This velocity will be either increased or diminished, accordingly as the upper hook declines more or less from a position perpendicular to the lower hook. In order to adapt the thimbles to these hooks;—as the lower hook is one inch and a half farther from the gateway than the upper hook, the lower thimble must have its eye an inch and a half farther from the heel of the gate than the eye of the upper thimble, in order that the bars of the gate may be in a horizontal position when it is shut. And, as the upper hook projects half an inch less from the hanging-post than the lower hook, the upper thimble should be fixed half an inch nearer the farther side of the heel of the gate than the lower thimble, in order that the gate may be in a perpendicular position when shut. If the thimbles have straps embracing the heel of the gate, and proceeding a few inches along each side of the bottom and top bars, and if they are fixed to the heel bars and braces, by means of iron pins and screw nuts, great firmness will be given to the gate at those two points, which are those that suffer the greatest strains.

\* \* To this communication are annexed a certificate from Mr. Edward Simpson of Wooden Croft Lodge, near Barnard Castle, in favour of the advantage of these gates in saving and durability; and also a letter from Mr. T. N. Parker, author of a well-known Treatise on the hanging of Gates, expressing his approbation of the same.

## VII.

*Description of an improved Gun-Lock, by Mr. GEORGE DODD.\**

Description of  
an improved  
gun lock, by  
reference to the  
drawings.

THE figures in *Plate I.* represent Mr. Dodd's improved gun-lock with its parts in their several situations and positions. The shaded drawing, *Fig. 1.* represents the exterior parts of the lock; and *Fig. 2.* represents its interior. *Fig. 6.* exhibits, in perspective, the tumbler, the sear and the sear spring in the position of whole cock. The outline plans 3, 4 and 5 shew the several positions of the parts, at full cock, half cock, and immediately after the discharge. The tumbler A terminates on the lower side in a tail, as usual; upon which the main spring acts: but, on the upper or opposite side, it is formed so as to have two notches or bents, one very deep for the half cock, and the other shallower for the whole cock, as is seen in the figures. The circle *a*, *Fig. 3.* supposed to be described by the extremity of the bearing face of the tumbler at whole cock, is larger than that through which the extremity of the bearing face of half cock passes, see *Fig. 5.* and the center of the sear B is placed in the outer of these two circles, having the under side of its nose fashioned in the arc of the circle *bb*, described by the motion of its extremity. The bearing surfaces of the bents or notches of half and of whole cock are made to fit this face; or, in other words, they form parts of the same circle, when respectively at whole or half cock. D is the trigger, so formed and placed that, at whole cock it trips or draws out the sear, with great facility and quickness, by the action of an inner slope or face lying in the direction of a radius of the circle it describes; (see *Fig. 4.*) But when at half cock, *Fig. 3.* its action, by means of an outer slope or surface (which lies intermediate between radii drawn from the centers of the sear and of the trigger to the inner point of their contact) is so far from discharging the motion, that it tends to keep the sear more strongly in its place. These actions and properties are sufficiently evident from the figures.

Enumeration of  
the good quali-  
ties of this lock.

The advantages of this lock are, 1. It is fully as simple in its construction, or rather more so, than the common lock, and is therefore no less cheap and easy to be cleaned by a common

\* From the Transactions of the Society of Arts for 1804.



soldier or workman: 2. It is discharged very speedily, and cannot possibly catch or hang at the half cock in the act of discharging: 3. The bearing parts at half cock are extremely strong and cannot miss their hold or be thrown out of taking by any accident. • In particular the trigger cannot be made to remove the sear; because its action at half cock is in the contrary direction. Hence it is much more simple and its means of safety are nearly as secure as any bolt, that is to say it is perfectly effectual as far as regards the trigger; though it does not, like some of the bolt stops, prevent the full cock being made. But on the other hand, as the inventor remarks, its security in no respect diminishes its ready use. For bolts, exclusive of the additional expence, have the disadvantage of requiring a previous operation before the piece is fit for service. Few people when alarmed have the presence of mind to unbolt, but they instantly attempt to cock. Disappointment tends to produce that agitation and confusion of mind which, at such a juncture, may occasion the loss of their lives from opponents who are little disposed to shew mercy to an enemy from whom they had no reason to expect any.

Certificates of the utility and novelty of this invention, from respectable makers, with letters of approbation from the Board of Ordnance, were exhibited to the Society of Arts; and it is probable that this apparatus will obtain the encouragement it appears to deserve. The Society expressed their sentiments by awarding the silver medal with the sum of ten guineas to the inventor.

### VIII.

*Investigation of the Properties of the Lines drawn in a Circle by Mr. BOSWELL in the Tenth Volume of this Journal. By Mr. JOHN GOUGH.*

To Mr. NICHOLSON.

S I R,

*Middlesex, April 17, 1805.*

THE theorems respecting the circle, given in your number for March last by the ingenious Mr. Boswell, will undoubtedly prove useful to the artist and practical philosopher. On this account

Propositions  
respecting the  
circle's area, &c.

Propositions  
respecting the  
circle's area, &c.

account they ought to be made as correct as possible; which has not been as yet done, either by Mr. Boswell, or by his more scientific commentator in your number for the present month. This declaration in a manner compels me to undertake the following investigation of the subject, in which I shall refer to *Fig. 4, Plate XI.* of your Journal for April, requesting the reader to place the letter T at the upper extremity of the vertical diameter, and C at the opposite end.

*Theorem 1st.* Let the circle ITFC have unity for its diameter; draw the diameters IF, TC at right angles to each other; bisect the radius TO in W; join IW, and produce it until it meets the circle again in B: these things being done, the square upon IB is equal to .8000; which exceeds .7854, or the common expression for the area ITFC by the fraction .0146.

*Demonstration.* Put the radius IO = R = .5; then OW =  $\frac{1}{2}$  R; since the triangle IOW is right angled at O, by hypothesis,  $IW^2 = R^2 + \frac{1}{4}R^2 = \frac{5R^2}{4}$ , *Euc.* 47.1. Now [the triangles OIW, BIF are equiangular; because the angle at I is common to both; and the angle FBI is equal to WOI, being right, *Euc.* 31. III; consequently, as WI : IO :: IF (= 2 IO) : IB; hence as  $WI^2 (= \frac{5R^2}{4})$  :  $IO^2 (= R^2)$  ::  $IF^2 (= 4R^2)$  :  $IB^2 (= \frac{16R^2}{5})$ ; but  $R^2 = .25$ ; therefore  $IB^2 = \frac{16 \times .25}{5} = 16 \times .05 = .8000$ . But .8000 - .7854 = .0146. Q. E. D.

*Lemma.* The area of the circle ITFC is equal to the rectangle under the radius OI, and the semi-circumference ITF; for this area is equal to a triangle having IO for its altitude, and the whole circumference ITFC for its base. (*Archimedes de Circulo Prop.* 1st.)

*Theorem 2nd.* If the right line IB be the side of a square, which is equal to the area ITFC; and BG be drawn perpendicular to the diameter IF; the segment IG of the diameter IF, cut off by BG, is equal to  $\frac{1}{4}$  of the circumference ITFC or the arc TBF.

*Demonstration.* The triangle IBF is right angled at B, 31 E. III. and BG is perpendicular to IF by hypothesis therefore

therefore the rectangle  $FI, IG = IB^2$ , 8. E. VI.; but  $IB^2 =$  Propositions respecting the circle's area, &c. the rectangle under  $IO$  and the arc  $IFF$  by lemma; consequently as  $FI (= 2IO) : IO ::$  arc  $ITF$ : right line  $IG$ ; 14 E. VI. hence  $IG = \frac{1}{2}$  the arc  $ITF$ , = the arc  $TBF$ , Q. E. D.

*Corollary 1.* If  $IG$ , a segment of  $IF$  be equal to the arc  $TBF$ ; draw  $GB$  perpendicular to  $IF$ ; and let it meet the circle in  $B$ ; the line  $IB$  is the side of a square; which is equal to the area  $ITFC$ . This is the converse of the Theorem.

*Cor. 2.* If any angle at the centre of the circle as  $IOB$ , be given in parts of the right angle  $TOF$ ; and  $IG$  be equal to the arc  $TBF$ ; find a right line  $N$ ; which shall be a fourth proportional to the angles  $TOF, IOB$  and the right line  $IG$ ; this line  $N$  is equal to the arc  $ITB$ ; and a mean proportional betwixt  $IO$ , and  $\frac{N}{2}$  is the side of a square which is equal to the area  $IOBI$ ; join  $IB$ , and from the last mentioned square take the triangle  $IBO$ ; the remaining magnitude is equal to the circular segment  $ITB$ .

*Cor. 3.* The square upon  $BF$  is equal to the difference of the areas of the circle and its circumscribing square.

*Problem.* If the circumference of a circle, whose diameter is unity be denoted by 3.1416; it is required to find a right line which shall approximate very nearly to  $\frac{1}{4}$  of this number, or .7854.

*Construction.* Draw  $IB$ , as in Theorem 1st, and make  $BG$  perpendicular to  $IF$ ; then  $IG \times I = IB^2 = .8000$ ; consequently  $IG = .8000$ , which is greater than .7854. Let the reader take  $g$  in  $IG$ , so that  $Ig$  may be of the required length; then as  $8000 : 7854 :: IG : Ig$ ; but 8000 is to 7854 nearly as 55 to 54; therefore divide  $IG$  into 55 parts and  $Ig$  will be 54 of these parts. Draw  $gb$  perpendicular to  $IG$ ; join  $Ib$ ; and the square upon  $Ib$  will be nearer the truth than that upon  $IB$ . If a more complete approximation be required, it may be discovered by the method given in the ninth Problem of Emerson's Arithmetic. Q. E. F.

It is the business of the practical geometrician to determine the value of these propositions in practical geometry. The ingenious Mr. Boswell considers the first theorem to be of utility; for which reason I imagine any improvement in the  
discovery



discovery will prove acceptable not only to the inventor, but to several of your readers who are artists and mechanical geometers.

JOHN GOUGH.

# IX.

*On the Culture, Properties, and comparative Strength of Hemp, and other Vegetable Fibres, the Growth of the East Indies. By Dr. WILLIAM ROXBURGH, of Calcutta\*.*

DEAR SIR,

YOUR letter of the 16th of May, 1799, I received on my return to Bengal in October last; but that from the Society established in London, for the Encouragement of Arts, &c. of which you are a member, is not to be found.

Ill advised experiments for the culture of hemp in India.

I was rather surpris'd, on my return to Bengal, to find the directors had sent out a person (Mr. Sinclair) to establish the cultivation of hemp, a thing I had begun some time before. Even on the coast of Coromandel, ten or twelve years ago, I made a most successful trial, the result of which was laid before that government, to be sent to the Honourable Court of Directors; and again in Bengal, since my appointment to the station I now hold. Mr. Sinclair is dead, and the experiment is still carried on in a most expensive manner; whereas it could be continued where it was first begun, in the botanic garden, at no expence, and with more prospect of success. Should government continue to be the cultivator, the price will be enormous. Eighty pounds weight is all, I believe, that is yet forthcoming, and costs from 10,000 to 20,000 rupees. Such experiments throw a complete check in the way of all attempts to introduce new, or improve old, branches of agriculture and commerce. A small premium should be offered to the natives, and honorary rewards to Europeans, after the example is set on a small and not expensive scale.

Best method of promoting its cultivation.

Botanic garden at Calcutta.

The botanic garden was at first made very large, four times more than was necessary for such a garden, the intention of

\* Communicated to the Society of Arts, 1804.

which

which was merely to make experiments, and to invite the natives to see and profit by the examples in husbandry carried on there.

A quantity of my bow-string flax was, I understand, sent from the coast about two years ago, for the directors. I could wish to know what was done with it; for, to me, it seems to be the strongest vegetable fibre we are acquainted with. I mean to send some by the January ships from hence, through the medium of this government, and wish it may fall into your hands, and that its qualities may be properly examined by the Society for the Encouragement of Arts. I am really sorry that the letter, inviting me to become an honorary corresponding member of that Society, should have been lost. I beg you will assure the Society, that I am sensible of the honour they have done me, and shall be very happy to have it in my power to contribute my mite to promote the views of that laudable institution.

Bow string flax is the strongest of vegetable fibres.

When any new object, promising to become useful in the arts or manufactures of our country is discovered, and reported to your Society (for example, the bow-string flax,) the Society will probably address the Court of Directors, and recommend the cultivation and importation into England of the commodity itself.

Another object, of more national importance, which I recommended to this government, before I went to the Cape, was the growth of one of the most noble of the palms, the arrow, mentioned in Marsden's History of Sumatra, page 77, and said to yield at an early age (from five to seven years,) fibres ready prepared by nature, flexible, strong, and most durable, and the most convenient for cables and cordage of all kinds, that can be desired. It also yields great abundance of its wine; palm wine, which can be converted into sugar or ardent spirits; and when the tree is old, its pith is the basis of the sago we so much value. I have distributed many hundred plants, and have still a great number in the garden\*, beside many thousand seeds in the ground. Drawings, and a description of the most valuable tree, were sent to the directors, under the name of *Jaguerus rumphii*; but as the trees from which they were taken have advanced in size and age, a new

Excellent qualities of the arrow palm;

and its sago.

\* Feb. 1801. About 100,000 plants have been reared in this botanic garden since the date of this letter.

set of drawings, and a new description, of the old one corrected, becomes necessary. They will be sent to the Directors this season. The object may be such as your Society wish to attend to; and on that account I have ventured to trouble you with the above statement.

I am, dear Sir,

Your most obedient Servant,

W. ROXBURGH.

Calcutta, Dec. 24, 1799.

Robert Wisset, Esq.

DEAR SIR,

Experiments on  
hemp.

MY letter of the 24th of December, 1799, I am afraid, has not reached you. The experiments on hemp, therein mentioned, have not, I believe, thrown much additional light on the subject. My friend, Capt. Burrows, of the Earl Howe, has done more to make our own indigenious species (the *sun* of the Bengalese) better known, than any other person I am acquainted with.

Comparative ex-  
amination of  
vegetable fibres.

For these last twelve months my attention has been much taken up in collecting and comparing the various vegetable fibres of Asia, &c. used for cordage, cloths, and paper. The result of these I have lately presented to the supreme government, to be sent to the Hon. the Court of Directors, in reply to the 79th paragraph of their general letter of the 7th of May last. This paper may be interesting to your Society, particularly at this time, when the attention of all good patriots is drawn towards the discovery of a substitute for Russian hemp. This paper, with my former essays, contain much information on the subjects therein mentioned.

Necessity of a  
substitute for  
Russian hemp.

The *sun* plant is  
most promising.

The *sun* of the Hindoos, which is the prepared fibres of the bark of a well-known Indian plant, the *crotalaria juncea* of Linnæus, still appears to me to be the most promising substitute for hemp which has come to our knowledge; I mean, when every circumstance, relative to its quickness of growth, its being already universally known and cultivated by all the nations of India, its low price, pliability, strength, durability, &c. &c. are taken into consideration. All that can be necessary for the procuring and transporting to England any quantity of this material, is to ensure the cultivator a certain



certain price, and ready market for the commodity; and to have it properly cleaned and packed, to render the freight as low and convenient as possible. Cleaned samples of this very substance were sent by me, six or seven years ago, to the Directors; so that the fault is not mine, if it is not already better known than it seems to be.

The discovery of a substitute for Russian hemp is certainly an object of the first magnitude. If *sun* is found to be the best substitute yet discovered, and costs in India, say, when properly cleaned, ten pounds per ton, and the freight sixteen pounds, there will still be a considerable profit to the merchant, particularly in times of war; for, I believe, it rarely happens that hemp sells so low in London as thirty pounds per ton. Should the subject appear of consequence to you, I beg you will call the attention of the members of your Society to it.

It can be afforded at a reasonable rate.

That I may not encroach too much on your time, I will close this letter by referring you to my friend Mr. Boswell, the late Marine Storekeeper and Naval Paymaster, for any farther information you may want. I send this package by him. Mr. Babb, late a Member of the Board of Trade here, can also give you much information.

I am, dear Sir,

Your most obedient Servant;

W. ROXBURGH.

Calcutta, Feb. 27, 1801.

Robert Wisset Esq.

*Observations on the Culture, Properties, and comparative Strength of Hemp, Sun, Jute, and other Vegetable Fibres. the Growth of India, communicated by Dr. ROXBURGH.*

HEMP, considered merely as an article of trade, is an object of the first importance to the merchant; but, when we reflect on its various uses, and observe that hardly any art can be carried on without its assistance, or of some other substitute; the objects it embraces are immense, and there are few that better deserve the attention of the philosopher or intelligent artist. Cordage makes the very sinews and muscles of a ship, and every improvement which can be made in its preparation, either in respect to strength, pliability,

Importance of hemp and its very extensive use in society.

or durability, or in bringing to light substitutes equally good or better, particularly where hemp itself cannot well be had must be of immense service, particularly to the mariner, and to the commerce and the defence of nations.

Its cultivation in our colonies intitled to support and encouragement.

The cultivation of this important plant in our colonies has not only, at all times, met with encouragement from the government, but also of late from the East-India Company in Bengal, where extensive experiments were begun by the late Mr. Sinclair, and after his death carried on by Thomas Douglas, on the culture of hemp and flax, on account of the Honourable Company. A clear and impartial statement of these trials is to be wished for, as it will, no doubt, throw much light on the cultivation of hemp in India, and enable us to proceed with greater prospects of success than ever.

It may be very beneficially produced in India.

My own experiments and inquiries on the same subject, both on the coast of Coromandel and Bengal, have been many, though not extensive. Their result leads me to think, that hemp may be cultivated to great advantage over the interior parts of Bengal and Behar, where the seed should be sown about the beginning of the periodical rains, or earlier, if there have been frequent showers, on elevated spots of rich loamy soil, such as the Ryots cultivate tobacco, sun, and paat on, near their habitations. In situations of this nature it thrives well, and will be easily attended to. At first, some encouragement will certainly be necessary, to induce the Ryots to undertake this new branch of agriculture. For, although the plant is perfectly familiar to every Hindoo, yet the cultivation on an extensive scale, for the fibres of its bark, is perfectly unknown to them. I would therefore suggest, that they should pay no rent for the ground so occupied for a certain period; that seed should be given gratis; that they should be ensured a certain price for the hemp; and finally, a reward or premium to the person or persons who produced the greatest quantity of the best hemp within a stated period.

The Hindoos cultivate it only for the seed.

Season for growing it.

In many parts of Bengal, particularly where the land is so low as to remain humid through the dry-weather season, hemp thrives luxuriantly during the cold season; but the water is then too cold for macerating the plants to the greatest advantage: one day in June, July, or August, has more effect in loosening the bark, than eight in December, January, or February; consequently, the prolonged immersion injures the quality

quality of the hemp much. The rainy season is therefore preferable for the cultivation and maceration, even if the plant grew better during the cold, which is by no means the case, particularly on lands elevated above the level of the annual inundation of the low rice-fields. We must therefore content ourselves with one crop in the year; for it is a very false notion, and a very prevailing one, that the fertile fields of Asia produce at least two crops annually; as well might we say, that the fertile lands of England yield at least two, because a well-managed garden, near London, or some other large city, will produce repeated crops in the year: so in India, by great care and industry, a spot here and there will produce two or more crops. The burning heats of Asia, while they last, are as unfavourable for vegetation, as the frosts of winter in Europe.

One annual crop only can be had.

Besides hemp and flax, the vegetable kingdom, particularly that natural division called by Linnæus, *Columniferæ*, abounds in plants which produce materials fit for cloths, paper, and cordage: almost every nation or country possesses something of the kind peculiar to itself. To ascertain what these are, as well as to find out new ones, to try their comparative strength, durability, texture, &c. has, at various leisure hours, employed my attention for many years past. Drawings and descriptions of many of them have been already laid before the honourable Court of Directors. There are, however, some others which remain to be brought under view and compared with the kinds we are best acquainted with, which I have attempted in the following experiments. Many other sorts are also mentioned by various authors and travellers, of which I know nothing more than the names. Two of these are mentioned by Marsden, at pages 75 and 76 of his history of Sumatra. Others are peculiar to Pegu, &c. &c. These I must omit for the present, and confine myself to such as I am more intimately acquainted with. The better to enable me to proceed in this inquiry, I have cultivated, in the Botanic Garden at Calcutta, many of the plants themselves which produce the materials hereafter mentioned; prepared their fibres in general by maceration, &c. as with hemp and flax in Europe. And, to compare their strength (plain, tanned, and tarred,) had then made into cords, composed of three simple yarns, as nearly of the same size and hardness as a

Other vegetables particularly the *columniferæ*, fit for the uses of hemp.

Descriptive outline of the author's experiments on various plants for cordage, &c. Preparation of the fibres.



Cordage manu-  
factured,

and tried in the  
white, and also  
tanned and  
tarred.

Tanning  
strengthens ani-  
mal fibres, but  
quere vegetable?

Hindoo rope-maker could make them; but, in spite of my utmost care, they were always too hard twisted to be of the greatest possible strength \*. Of each sort there were six, when there was a sufficient quantity of the fibres. Three of them were about the size of a log-line, and three a size larger than a whip-cord; one of each size and sort was kept white: the average number of pounds which broke them, (for repeated trials were made, and always with lengths of exactly four feet), will be found in the first and second columns on the right. One of each was tanned† with the astringent fruit called *gaub* by the Bengalese, (*Embryopteris Glutinifera*, Roxb. *Coromandel Plants*, Vol. I. No. 70.) Their strength is represented in the third and fourth columns of the following table. And, lastly, one of each sort was tarred: their respective strengths will be found in the fifth and sixth columns.

We know the tanning principle strengthens the fibres of leather, (animal fibres,) but are not so clear that its operation on vegetable matter is uniformly the same. The attention bestowed to ascertain this point in these experiments will, at least throw some light on the subject; and may induce others, better qualified, to extend the inquiry (here in India), where tanning materials abound.

\* The experiments of Reaumur, Sir Charles Knowles, and Du Hamel, uniformly prove, that when hemp-rope is twisted to the usual hardness, which is that which brings them to two-thirds of the length of their respective yarns, their strength is lessened by nearly one-fourth, when compared with ropes consisting of the same number of the same yarns twisted up to only three-fourths of their length. There will be no difficulty in accounting for this difference, if we consider that a skein of fibres may be twisted so very hard, as to break with any attempt to twist it harder. In this state the fibres are already strained to the utmost, and cannot support any weight or additional strain.

† The idea of tanning cordage is far from being new; for the fishermen of Asia, as well as of Europe, not only tan their nets and lines, but also their sails, to give them additional strength and durability. The same process might be productive of the same effects, if employed on cordage made of the materials (No. 2, 5, 6, 7, 8, and 15), specified in these experiments, which induced me to recommend its being tried with sun cordage, in my letter to the Board of Trade, in August, 1797.

Another

Another point of the utmost importance to be ascertained is whether tanned ropes will be preserved by the tan, with which they are impregnated, when flowed away wet; as tarred ropes are preserved by the tar when so circumstanced. It is nevertheless a well-known fact, that tarred cordage, when new, is weaker than white; and that the difference increases by keeping. Tar can therefore only be employed to preserve cordage, and not to strengthen it; so that if tan will add strength, or even not weaken vegetable fibres, and at the same time preserve them; of how great advantage to the nation would the discovery of a convenient practicable process be. For all cordage, exposed to be alternately very wet or dry, requires to be impregnated with a preservative. And, to conclude this long note, I beg leave to observe, that tar is not the produce of the warmer parts of Asia. Are we, therefore, to conclude, that no material, the produce of these parts, can be applied for the same end? Let us not entertain any such idea. Nature is abundantly kind, and furnishes every country and climate with what is most proper for the use of its inhabitants.

Whether tan will preserve wet rope?

Tar does not strengthen cordage; but the contrary.

Tar not produced in tropical Asia.

The annexed statement of the experiments made on the substances there specified, can only be deemed an attempt towards ascertaining their relative strength; and though they are the average result of several trials made on the strength of each cord, plain, tanned, and tarred, yet I must acknowledge they cannot be deemed any thing more than a first essay, chiefly owing to the lines being in general ill laid, some more and some less twisted, and by no means to be compared with those of Europe. For in some instances, I found a small one sustain a greater weight than a much larger, made exactly of the same materials. It is therefore my intention to repeat them on a larger scale, and, if possible, with better made lines; for every thing depends on their being exactly of the same size and degree of twist.

Statement of experiments on the cordage.

## COMPARATIVE STATEMENT OF THE EXPERIMENTS.

NAMES of the PLANTS, and brief REMARKS on the various MATERIALS.  
employed in these EXPERIMENTS.

	Average weight at which each fort of cord broke.					
	White.		Tanned.		Tarred.	
	Large. Small.		Large. Small.		Large. Small.	
No. 1. English Hemp: a piece of new tiller-rope, opened out and made into cords, like the rest,	105	65	..	..	..	..
2. Hemp (Cannabis), the growth of this season, from the Company's Hemp Farm, near Calcutta,	74	50	139	60	45	46
3. Coir: the fibres of the husk of the Cocoa-nut; much used for cables and cord- age over Asia,	87	60	..	..	..	..
4. Ejoo (Saguerus Rumphii): the black horse-hair-like fibres, which grow round the trunk of this species of Sage Palm,	96	79	..	..	..	..
5. Robinia Cannabina, Dansha of the Bengalese: the fibres of its bark, prepared by maceration from the plants that had nearly ripened their seed, they are then of a dusky grey colour, and harsh nature,	88	64	101	55	84	39
6. The fibres of the bark of No. 5, prepared by maceration, from plants coming into blossom; at which time they are beautifully white, soft, and glossy,	46	20	61	35	48	30
* 7. Crotolaria Juncea, Sun of the Bengalese: the fibres of its bark, and universally known over India,	68	47	69	55	60	37
8. Corchorus Olorius, Bughhi-paat of the Bengalese: the fibres of its bark, called Jute by the same people,	68	47	69	59	61	36
9. Corchorus Capsularis, Gheec-nalta-paat of the Bengalese: the fibres of its bark they call Nalta Jute,	67	47	..	..	..	..
10. Flax (Linum Usitatissimum): the growth of the Company's Hemp Farm, near Calcutta	39	37	..	..	..	..

\* A cord, a very little thicker than a log-line, made of sun sail twine, broke with 148 pounds when dry;  
but, on being soaked in cold water for 24 hours, it bore, while wet, 222 pounds. This difference requires  
to be farther inquired into.



NAMES of the PLANTS, and brief REMARKS on the various MATERIALS employed in these EXPERIMENTS.	Average weight at which each sort of cord broke.					
	White.		Tanned.		Tarred.	
	Large.	Small.	Large.	Small.	Large.	Small.
11. <i>Agava Americana</i> : the fibres of its leaves. They are of a coarse harsh nature, and white, — — — — —	110	71	79	78	78	38
12. <i>Aletres Nervosus</i> —In Sanscrit, <i>Murva</i> ; <i>Murga</i> of the Bengalese: the fibres of its leaves made into these cords, after having been kept above one year, — — — — —	120	52	73	42	48	43
13. <i>Theobroma Augusta</i> , Linn.— <i>Abroma Augusta</i> , Hort. Kew.— <i>Abroma Wheelerii</i> , Kæn.—Woollet-comal of the Bengalese: the fibres of its bark prepared by maceration, &c. like hemp, — — — — —	74	61	58	51	44	29
14. <i>Theobroma Guazuma</i> , Bastard Cedav: the fibres of the bark of some straight, luxuriant, young plants, — — — — —	52	48	..	47	45	30
15. <i>Hibiscus Tiliaceus</i> , Bola of the Bengalese: the fibres of its bark, and employed for cordage by the inhabitants of the South-Sea Islands, &c. — — — — —	41	..	62	39	61	..
16. <i>Hibiscus Manihot</i> , a tall white-flowered variety: the fibres of its bark, which are beautifully white, glossy, and soft, — — — — —	61	..	..	..	..	..
17. <i>Hibiscus Mutabilis</i> : the fibres of its bark, and they are of a harsh nature and ill coloured, — — — — —	..	45	53	46	..	..
18. <i>Hibiscus</i> , a new species, from the Cape of Good Hope, said to be a native of Caffraria, where the fibres of its bark are spun, — — — — —	..	22	..	..	..	..
19. <i>Bauhinia</i> , a large scandent species: the fibres of its bark, cleaned without maceration, and used to make ropes, &c. of by the people of Napaul, where the plant is common, — — — — —	69	39	..	..	..	..
20. The same as No. 19, only maceration was used to help to take the bark off the twigs with more care, — — — — —	56	41	..	..	..	..
21. <i>Sterculia Villofa</i> : the fibres of its bark. Cords are made of them by the natives of the eastern frontier of Bengal, to bind wild Elephants, when first taken, — — — — —	53	..	..	..	..	..

The cords, when the trials were made, had been kept about six months after they were prepared, chiefly with the view of allowing the effects of the tan and tar to take place. The result of these experiments show, that tan has in general added strength, while tar has had a contrary effect; and in no instance is this more clearly evinced, than in the common hemp (*Cannabis*) cultivated in Bengal.

**Ejoo and Coir.**

To Ejoo and Coir, neither tan nor tar seem applicable; and in several of the other experiments, I had not a sufficient quantity of the materials to make the necessary number of cords, viz. six of each sort, to try with tan and tar, as well as in their natural state. At some future period, I hope to be more fortunate in procuring larger supplies of the materials, and also to add some other other sorts, such as the—

**Bow-string fibre.**

Rajemahl bow-string fibre, the produce of a new species of *Asclepias*, discovered by William Roxburgh, junior, amongst the Rajemahl Hills;

**New Zealand hemp.**

New Zealand Hemp;

*Hibiscus Cannabinus*, and some other of the same natural order (*Columniferæ*); (for in general their barks abound with strong fibres; witness the foregoing table, where six of them are to be found);

The leaves of a new species of *Andropogon*, &c. &c.

## APPENDIX,

*Containing Remarks on some of the Plants mentioned in the foregoing Table.*

**Remarks on hemp.**

No. 2. Hemp, or *Cannabis Sativa*.—Banga, in Sanscrit; Bunga, Bunch, or Bung, of the Hindoos; Bang, of the Persians; Kinnub, of the Arabians, is no doubt our own famous plant, now so common and useful in Europe. I have at different times examined various figures and descriptions, as well as the plants reared from Europe seed, comparing them with our Indian plant through its various stages, and can discover no difference whatever, not even to found a variety on. Perhaps few vegetables, so widely diffused over almost every part of the known world, and under the immediate manage-

It is well known in India; but not for its fibres;

ment of man, have undergone less change. It is perfectly familiar to all the nations of India, I may say of all the warmer parts

parts of Asia; yet I cannot discover that the fibres of the bark have ever been employed for any purpose. It is cultivated in small quantities every where, on account of its narcotic qualities.—The leaves of the male plant, and flowers of the female, are the parts in most general use.

I have repeatedly applied for the seeds of all plants reared in China, and other countries to the Eastward of the Bay of Bengal, as well as to almost every other part of India we have any communication with, for an account of the plants employed to supply materials for clothes and cordage, and for their seeds; but could never learn that Cannabis was one of them; nor were its seeds ever sent to me as such. nor in China,  
&c.

No. 4. The great strength of this substance makes it a very desirable object. For a description and drawing of the tree I refer to those \* which accompanied my letter to the Most Noble the Governor-General in Council, under date the 2d of January, 1800. The fibres employed in these experiments were taken from trees growing in the Botanic Garden at Calcutta, where they thrive well. I could observe, during the trials made in breaking the cords of this substance, that they were not so elastic as those of Coir, which will probably render it less fit for cables, but better for many other uses. Coir is certainly the very best material yet known for cables, on account of its great elasticity and strength. Ejoo; very  
strong, but not  
elastic.

Nos. 5, 6, 7, 8, and 9. These four plants have already been figured and described by me, in a memoir sent through the Governor-General in Council, to the Hon. the Court of Directors, in December, 1795. Since writing that paper, I have learned, that *sun* (*Crotalaria Juncea*) is almost universally employed, over the warmer parts of Asia, for cordage. On the Malabar coast, I find it is generally named by the gentlemen at Bombay after the province where reared. It is used in that place for lacing their cotton bales, on account of its great strength. Samples of three sorts, viz. Malwan, Rajapore, and Salsette, were sent to me, from them, by Dr. William Hunter; and am induced to think, little or no maceration is Other plants:  
  
The *sun* or *crotalaria juncea*.

\* A former set, No. 1179, sent in to Government on the 23d of November, 1797, were not so correct as could be wished, on account of their having been taken from young trees, just coming into blossom the first time.

employed



employed in taking the bark from the stalks, or in cleaning the fibres, which may add to its strength: for certainly maceration, particularly if long continued, must weaken fresh vegetable fibres considerably. The same gentleman sent me seeds of the Salfette sort: they have produced plants now in blossom, and from them have ascertained the identity of this species.

In some parts of Bengal, a most luxuriant variety is cultivated immediately after the rains, which often grows to the height of twelve or fourteen feet; while the common sort is generally reared in Bengal during the early part of the wet season, and grows to only about half the height of the former.

Danisha of the  
Bengalke;

I must farther observe, that the fibres of No. 5, possess great strength, and it seems to me to be one of the most fit of any of our Indian productions for cables and cordage. The plant grows generally to the height of from six to ten feet, the fibres long, but harsher than those of hemp, if not cut at an early period. It is very generally cultivated about Calcutta during the rains. An acre yields of the half-cleaned substance (the state in which the natives carry it to market), about 600 lbs. weight, and sells for about a rupee and a quarter per maund of 80 lbs.

beautiful but  
weak fibres.

No. 6. By cutting No. 5, the last-mentioned plant, when beginning to blossom, we have the most beautiful shining white fibres that can well be conceived, but (by my experiments) greatly weaker than when the seed is suffered to be nearly ripe before the plants are cut.

Flax cultivated  
in India for its  
seeds only.

No. 10. Flax, the plant, is very generally cultivated during the cold season, over the interior parts of Bengal and Behar, merely for the seeds, from which oil is obtained. The flax itself the Hindoos set no value on; for, after they have gathered the seed, they throw away the stalks as useless, having no knowledge of the fibres which their bark yields. Samples of the flax have repeatedly been procured by the Board of Trade, and sent to England to the Hon. Court of Directors; so that it is from England we may expect to learn its properties. If the flax has been found good, large quantities may be reared at a small expense; as the seed alone, which the crop yields, must be more than equal to the charges, to render it profitable to the farmer.

No.

No. 11. This *Agava* is of slow growth; on that account I doubt if ever it can be advantageously cultivated; but, where found wild in plenty, it may be manufactured at a trifling expense. Its great strength renders it an object worthy of attention. The fibres are coarse, consequently rope made of them harsh to the feel. *Agava Americana; of no great promise.*

No. 12. Drawings, and a description of the plant *Aletris Aletris nervosus*. *Nervosus Roxb.* the method of extracting the fibres, with a quantity of the substance itself, were sent to the Hon. the Court of Directors, through the Madras Government, above ten years past. I also gave a large quantity to Mr. Bebb, when he left Bengal in January, 1800, to take to England with him for trial there. The plant grows fully as well, and is as common as on the Coromandel coast. There has lately been about a biggah (third of an acre) planted out with it in the Botanic garden, the better to determine the expense, and the annual produce of any given quantity of ground.

No. 13. This plant, a native of various parts of India, New South Wales, Philippine Islands, &c. has been long known to botanists; yet I cannot, with all the attention that I have been able to bestow on the subject, find that the fibres, so abundantly interwoven through its bark, have ever been used or even taken notice of by any other person; so that I think we may look upon it as a new discovery, deserving of more than common attention, on account of the beauty, fineness, and strength of these fibres. *Abroma augusta,*

It is perennial, grows luxuriantly in the Botanic garden, and has been cut down twice within these six or seven months; so that I think it will, at least, annually afford two or three crops of shoots fit for yielding this substance. My experience does not yet enable me to state how much may be the yearly produce of an acre, but can venture to prognosticate as large a produce as can be obtained from an acre of Dancha, Jute, Sun, Hemp or Flax. *Its growth.*

To render this bark separable from the half-ligneous shoots it covers, to soften its external lamina, or epidermes, and the parenchymatous substance which firmly connects the fibres in their natural state, maceration in stagnant water for from four to eight days, during the warmer parts of the year, answers well whilst three times as many days are scarce sufficient during the cold season; indeed, the process is scarcely practicable then; besides, the fibres are greatly weakened by the length of the maceration. *Treatment to obtain the fibre.*

Immediately

Immediately on being taken out of the water, and while wet, the shoots are singly taken in the hand, rubbed with some coarse materials, such as a little dry grafs or gunny, to remove the exterior pulpy lamina or epidermis of the bark, which is destitute of fibres. This part of the process is easily effected; and when done, the clean shoots are to be made up into small bundles, and placed under weights, or some other equal pressure, to keep them firm at the middle and top, either under the surface of the waters, or out of it; the fibrous bark is then separated with the fingers from a small portion at the end of the ligneous shoot or stalk, which the operator takes hold of, and draws out one by one; when these are removed, the pulp, or parenchyma, which fills the interstices between the fibres, and connects them together, forming in the living plant that part of the bark which may be called its inner lamina, or cellular tissue, is immediately washed out in cold water, and the clean fibres spread out in the sun to dry. Such was the simple process by which this substance (which may very properly be called Indian Hemp or Flax) was prepared.

I have now under cultivation about the third of an acre (a biggah) of ground in the Botanic garden with this plant; the result shall be carefully noted from time to time. It might have been prudent to have withheld this account until that time; but the strong desire of making known a discovery, which may in a short time become beneficial to the public, induces me to be thus precipitate.

For a farther account of this plant, I beg leave to refer to my drawing and description thereof, sent to the Hon. the Court of Directors some years ago, and numbered 415.

W. ROXBURGH.

*Botanic Garden, near Calcutta,*

*Jan. 31, 1801.*

#### *Additional Experiments on the Strength of Sun, (No. VII.)*

Additional experiments on the strength of sun.

Some tanned sail-twine, made of this substance four years ago, for the inspection of the Marine Board, was made into a cord of three strands; each of the strands composed of four threads of the sail-twine.

Some more of the same sail-twine, tanned twelve months ago, was made into a similar cord, and another was made of the white unprepared twine.

The



The first, which had been tanned four years, broke with 110 lbs. when dry, and with 130 lbs. after having been steeped in water 24 hours.

The second, which had been tanned one year, broke with 123 lbs. when dry, and with 140 lbs. after steeping 24 hours.

The third, or plain white, broke with 148 lbs. when dry, and with 222 lbs. after steeping 24 hours.

### X.

*Description of the Bavarian Method of evaporating Saline Waters.* By M. BONNARD.

THIS new method, practised in Bavaria, has been introduced into the salt work of Moyenvie, by M. Cleifs, inspector of the salt-works of Bavaria.

Bavarian method of evaporating salt waters.

The pans are composed of square plates of cast iron, of 4 millimetres in thickness, and 4.76 centimetres on each side. These plates are joined by their edges, which are turned downwards, and consequently without the pan: they are solidly united by a piece in the form a square gutter which receives the edges, and is secured by a great number of screws.

An evaporating house is composed of six pans, of this construction, disposed in two rows; but these pans have different uses, which require a particular arrangement.

That in the middle of the back row is the smallest; it has no particular fire-place, but it is heated by the junction of the chimnies from the other fire-places. The salt-water deposits its impurities in it; it is called the small pan.

From the small pan the salt water passes into the graduating pan, which is lower than the first, and placed in the middle of the front row; it is there kept in a state of constant ebullition: the water is concentrated in it to 20 degrees, and deposits a part of its sulphated lime.

From the graduating pan the salt water passes into the preparing pans, which are lower than it, and situated at the two extremities of the back row: there it is also kept constantly boiling,

Bavarian method of evaporating salt waters.

boiling, it is completely concentrated, and deposits all its sulphate of lime; it is then passed into the crystallizing pans, still lower than those of preparation, and placed at the two extremities of the front row: there the water scarcely boils, and the salt crystallizes.

Each pan, with the exception of the small pan, has a particular fire-place, the chimnies of which pass round the sides of the pan: they unite under the small pan, by which means there is little heat lost.

These pans are placed two and two in chambers of wood, the joinings of which are well secured, which close them hermetically: these chambers are low, and their ceilings are perforated in the middle with holes terminating in a tube, by means of which the aqueous vapour is disengaged with rapidity. The chambers for the preparing and crystallizing pans have their ceiling pyramidal, or in the form of a reverse hopper, while that for the small pan and the graduating pan is horizontal.

The saline waters are passed successively into these four kinds of pans; the workmen penetrate into the chambers, in the midst of the vapour, to open the communications. This operation is performed every six hours, and the water in each pan is restored to the level at which it stood six hours before. Every three hours the salt in the crystallizing pans is collected, it is brought with scoops to elevations on the front edge of the crystallizing pans, where it drains; it is afterwards carried into drying rooms, which surround the outside of the chambers: these are spaces covered with iron plates; they are warmed by heat-tubes leading from the fire-places.

Every eight days they take away the sulphate of lime, throw out the mother-waters; and break the shell, that is to say, the incrustations of salt which adhere to the bottoms of the pans: every twenty-four days the work is entirely stopped to repair the pans; an operation which is performed by the workmen themselves.

Economy of fuel.

It has been found that this method of evaporation affords a saving of more than one-third of the fuel.

An improvement has lately been made in this process at Dieuse: the small pan has been suppressed, and the drying rooms have been replaced by auxiliary pans, in which a coarse salt is made.

The

The heated drying rooms are useless when the humidity of the salt arises from the muriate of lime it contains.

Bavarian method of evaporating salt waters.

*Explanation of the Plates III. and IV.*

*Fig. 1. Plan of the pans.*

No. 1. Small pan.

No. 2. Graduating pan.

No. 3. Preparing pan.

No. 4. Crystallizing pan.

The disposition of the plates of iron which compose these pans, is shewn in No. 2.

*a, a.* Elevation on which the salt is placed to drain, as it is taken from the crystallizing pans.

*b, b, b.* Wooden partitions which separate the chambers.

*c, c, c.* A raised wooden ledge which surrounds the pans.

*Fig. 2. Section of the evaporating chamber which contains the pans 1 and 2.*

*d, d, d.* Heat tubes which give heat to the small pan, and contribute to heat the others.

*e, e, e.* Fire place for the pans.

*i, i, i.* Pillars of cast iron under the gratings *g, g, g*, which support the bottom of the pans.

*h.* Wooden chamber which contains the two pans.

*k.* Opening by which the vapours escape.

*Fig. 3. Section of the evaporating chamber which contains the pans 3 and 4.*

*a.* Elevation on which the salt from the crystallizing pans is placed to drain.

The other letters indicate the same parts as in the preceding figures.

*Fig. 4. Method in which the plates of iron are joined to form the pans.*

*a.* The iron plate.

*b.* The iron gutter which receives the edges of the plates, and is strongly fastened with screws.

*i, i.* Pillars of cast iron which support the bottom of the pan.



## XI.

*Letter of Inquiry respecting the late Dr. IRVINE'S fundamental Experiment on the relative Capacities for Heat of Ice and Water. With an Answer by Mr. IRVINE.*

Request that Mr. Irvine would describe his father's experiment on the capacities of ice and water.

Change of form would influence the result.

A CORRESPONDENT would be much indebted to Mr. Nicholson if he would convey by publication in his Journal, or otherwise, the following request to Mr. Irvine. That gentleman in his paper in the Journal of last month \* mentions his having in his possession the experiments of Dr. Irvine on the capacities of bodies. It has always been a wish with these chemists who have attended particularly to this subject to know in what manner Dr. Irvine made the experiment to ascertain the comparative capacities of ice and water. If either of the bodies during the experiment change its form, if the ice were melted, or the water congealed, it becomes less decisive, as it may be objected by those who maintain the opinion that latent caloric exists in bodies either in whole or in part in a state of chemical combination, that the result might arise from such a combination, and not from a change of capacity. But if the experiment were made in such a manner that no change of form took place, which though difficult is possible, Dr. Irvine's theory, which is so much superior to the other, is unequivocally established. It would be conferring a favour on the chemical world, if Mr. Irvine would take the trouble of saying in what manner the experiment was conducted, providing such a notice would not interfere with the intention he has announced of giving a more full account of his father's investigations, an account which would be eagerly received by chemists.

*September 24th, 1803.*

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*Reply by Mr. IRVINE.*

The experiments may be made without being liable to the objections before stated;

WITH regard to the enquiry of Mr. Nicholson's correspondent, it would give me pleasure to inform him of any circumstance within the sphere of my own knowledge that should tend to add to the illustration or proof of my father's theory.

\* Sept. 1803. See our Address to Correspondents of last month.

There

There does not seem to me any difficulty in explaining satisfactorily how experiments on the capacities of ice and water may be conducted without being exposed to the objections above stated; at the same time, though these experiments may be conclusive as far as they go, I have not been accustomed to consider them as altogether so decisive of all arguments upon this subject. —but may not be unexceptionable.

In a general way then the experiments of Dr. Irvine were conducted in the following manner. The capacity of water being taken at unity, pains were used to ascertain the capacities of mercury, river sand, pounded glass, and iron filings, with respect to water, and consequently to each other in the ordinary manner. It is at present of no importance what proportional quantities of the materials were employed. This being done, the capacity of one of these substances was experimentally compared with that of pounded ice or snow. —

Every precaution was used to ensure success. The weight and capacity of the vessel was determined, and the colder and hotter body alternately added to the other. The temperature of the air was always below 32°, as was that of all the materials and of the vessel. No water could therefore be formed. One of these was then examined against pounded ice at a temperature of the place always under 32°.

In his first experiments I believe Dr. Irvine used mercury, but afterwards I know that he preferred iron filings and sand. For example, if the vessel and room were at 11°, let half an ounce of powdered ice from distilled water, at temperature 30° be poured on four ounces of iron filings at 11°, let the temperature of the mixture be noted after stirring, the due allowances made for the heat gained by the vessel and the air, and a proper calculation made. Let this experiment be reversed by cooling the ice to 11°, and pouring the iron filings at 30° upon it, and let a calculation from this be compared with the former and corrected by it. Finally, let these experiments be compared with others where different quantities of materials are used, and of different temperatures, and you have a view of the method employed for determining this point by Dr. Irvine, which does not appear to be objectionable upon other grounds than all experiments for ascertaining capacities are, none of which have any pretensions to perfect accuracy. Dr. Irvine was far from being satisfied that his experiments were mathematically precise. But he uniformly found the capacity of ice to be less than that of water, and that in a greater ratio than is generally allowed.

Example.

The common notion, of ice and water being directly applied to each other, unfounded and impracticable.

With respect to the common notion that experiments on the capacity of ice are made by mixing it with water, it is altogether false in the case of Dr. Irvine. No doubt this may be done theoretically. It is easy to say that ice loses so many degrees which heat water only so many. But then no ice must be melted, which if not impossible is extremely difficult, or what is equally so, the quantity melted must be found, and an allowance made for the latent heat, which is itself not precisely ascertained, at least not with sufficient accuracy for this purpose. I have only to add that this gentleman's enquiries would have been earlier noticed if I had been informed of them, which I was only a few days ago.

W. IRVINE.

April, 1805.

## XII.

*Short Account of some of the most remarkable Facts and Observations in an Aerostatic Voyage, made from Peterburgh, by Messrs. ROBERTSON and SACHAROFF, under the Sanction of the Imperial Academy. W. N.*

Aerostatic voyage from Peterburgh.

THE notice of an aerostatic voyage performed by Messrs. Robertson and Sacharoff from Peterburgh, under the direction of the Imperial Academy of Sciences, June 30, 1804, of which an abridged account was given to the National Institute of France\*, is very interesting, from the scientific views and conduct of the managers Lowitz and Robertson, as well as for its other particulars.

Ascent of the balloon with two observers.

Their balloon was a sphere of 30 feet diameter, and rose at a quarter past seven in the evening, with the ascensional force of one pound, the whole weight of solid matter (including 110 pounds of sand for ballast) being 622 pounds. When they were over the river Neva, at the elevation of 108 toises or 520 feet, they descended a little by the condensation of the gas; but rose again by throwing out a little of the ballast. The usual phenomenon of a slow rotation of the balloon presented itself, which doubtless arose from the unequal action of the air against an irregular surface, as we see in most other bodies rising or falling in a fluid.

\* Inserted in the Annales de Chimie, LII. 121.

But



But one of the most striking circumstances attending this voyage is the rational means which these philosophers made use of to determine, and in a certain degree to regulate their course. They made use of two instruments, a log and a telescope. The log consisted of two sheets of very thin paper, blacked, and fixed at right angles to each other by a very light cross of wood. This was suspended from their car by a string of sixty feet in length, and affording a different resistance to the air from that of the balloon itself,† it was found to draw the string out of the perpendicular direction, or as the narrators say, *to follow the balloon*: so that by its position determined by compass they could ascertain what direction they were pursuing. It also shewed by its relative rise and fall whether the apparatus was descending or ascending, before their barometer had indicated the slightest change.

Their direction  
observed by a  
floating log;

—of paper, &c.

Their telescope was applied to shew the direction of their course, and must have been much less subject to doubt than their log. Its application would be universal and perfect, if the earth could be seen at all times from the elevated regions of the air. It was directed perpendicularly downwards by means of a plumb-line, and having a considerable magnifying power, the objects upon the surface of the earth were seen moving across its field of view, and their direction would most clearly ascertain that of the car itself, and also its velocity. If, for example, the magnifying power were fifty times and the field of view one degree, the visible space included in that field from an elevation of two miles would be about 180 feet in diameter, in which objects of six or seven inches broad might be very well distinguished through a favourable atmosphere; and at so low a velocity as one mile an hour the whole field of view would be passed over in about twelve seconds. Hence we see that the method affords a considerable degree of accuracy, and will not in general require any great power of magnifying or delicacy of observation. The computation would be founded on the following problem, which will not present any difficulty to those who are acquainted with these subjects, if the physical allowances for temperature in barometrical admeasurements be admitted to be correct enough for this purpose.

Perpendicular  
telescope by  
which their  
course and velo-  
city were seen  
on the ground.

Investigation of  
the degree of  
accuracy of this  
method.

† Or rather, perhaps, because not exactly in the same current of the atmosphere. N.

Given

Problem for  
computing the  
velocity of a  
balloon by obs.  
through a tele-  
scope.

Tabulated results  
for practice.

The observed  
course, &c.

A pigeon let  
go.

The dip of the  
magnetic needle  
is less at great  
heights in the  
air.

This may afford  
means of shew-  
ing the heights,  
&c.

Singular echo  
of the voice  
heard from the  
earth at the dis-  
tance of two  
miles.

Given the temperature on the earth and in the car, the height of the barometrical column, and the time employed in the apparent transit of an object on the earth through a given angle or field of view; to find the velocity of the observer.

For practice it would perhaps be sufficiently exact and convenient to compute a small table, in which, neglecting the temperature, the velocity in miles per hour might be had by inspection, when the height of the mercury, the time of transit, and the magnifying power were known.

The aeronauts having noticed by their instruments what were the direction of the currents of air at different heights, found themselves in one which carried them directly towards the Baltic. They therefore descended till they saw by the barometer that they had returned to a current which carried them inland; and afterwards again rose much higher, and saw with great precision by their telescope the instant of their quitting the gulph. When the barometer stood at 24 inches they let go a pigeon, who flew with difficulty and would not quit the balloon; but upon being precipitated he in vain endeavoured to regain it, and at length descended rapidly towards the earth.

At ten at night the balloon had risen to an height indicated by 22 inches of the mercurial column, the thermometer standing at  $4\frac{1}{2}$  degrees (I suppose centigrade). Here it was that M. Sachatoff carefully observed a phenomenon which had been before remarked by M. Robertson in his ascent from Hamburgh, but at a much greater elevation. Their dipping needle was deranged; but on inspecting the common compass, its needle was found to be no longer horizontal, the north end being elevated near 10 degrees. On this phenomenon they remark that the magnetic attraction probably diminishing as the square of the distance may afford additional means of directing future observers in the atmosphere, and even determine the elevations independently of the barometer. From the present elevation a pigeon being thrown down, fell so directly that it was doubted whether he could have reached the earth alive.

Darkness coming on, it became necessary to descend, during which the observer repeatedly made an experiment which also promises to be of great utility to voyagers in the air, as well as to enlighten our conclusions respecting the phenomena of sound. When they spoke through a trumpet directed towards the earth, the voice was returned with extreme precision and

without



without seeming to have lost any part of its intensity. No re-  
 petition was made except when the trumpet was directed to  
 the earth; and the intervals of reflection were different ac-  
 cording to the elevation of the observers. The percussion im-  
 pressed on the air by the sound every time produced a slight  
 undulation in the aerostat; whence they deduce an inference  
 in favour of the supposed efficacy of cannon in partly modify-  
 ing or averting the discharge of stormy clouds. In one of their  
 experiments, the sound employed ten seconds in its return,  
 which would give a distance of about two miles out and home,  
 if the same law of the velocity of sound were supposed to  
 prevail in the perpendicular course as along the surface of the  
 earth, which however does not seem likely. The barometer  
 stood then at 27 inches, and at their outlet it was at 30 inches  
 on the ground. It would be easy, and it is surely desirable to  
 make experiments with cannon and stop watches on the velo-  
 city of ascending, and if possible, descending sound.

This reflection of sound or echo is a subject of very great  
 curiosity. There is perhaps no other instance in nature where  
 so extended a wall of reflection can be had. I am disposed to  
 think that the apparent intensity of the returned sound may in  
 some measure have depended on the perfect silence in which  
 the speakers were placed. In a still night the centinels on the  
 ramparts at Portsmouth may be heard at the Isle of Wight,  
 over a distance of five miles, and there are numerous instances  
 of low sounds, such as the beating of a clock or watch, or the  
 sounds of footsteps being heard to considerable distances, when  
 other sounds do not act on the organ of sense.\*

In their descent to the earth they passed through various  
 strata of vapours, all of different temperatures, and at the in-  
 stant the earth came in sight, the thermometer started up through  
 several degrees, probably because they had quitted a cold mass  
 of vapour which obscured their view, or perhaps because the  
 radiant heat of the earth's surface might at that moment have  
 reached them unimpaired.

\* See a curious paper of M. Perrole on sound, with the annota-  
 tions thereon, in the first Vol. of our quarto Journal, page 411.—N.



## XIII.

*Letter from Mr. CUTHBERTSON, containing Remarks on Mr. WM. WILSON's and Mr. HAUVY's Experiments on the Electricity of Metals.*

To Mr. NICHOLSON.

London, 54, Poland-Street,

April 24, 1805.

DEAR SIR,

Mr. Wilson's experiments on electricity;

of copper sifted through zinc,

controverted by another experiment of contact and separation.

Contact and separation produce the electric state;

but neither act will do alone.

IN your valuable Journal for January, page 42, you have favoured us with a letter from Mr. Wilson, containing some experiments which he calls, *exhibiting the electricity of metals*. Mr. Wilson says in his table of experiments, that copper filings sifted through zinc is positively electrified: this is an error which I thought myself obliged to take notice of, as it is a direct contradiction to what I have asserted in *my examination of Sig. Volta's experiments, which he calls fundamental, and on which his theory of galvanism rests; see this Journal, vol. II. page 281*; I have said therein, that if a plate of zinc be separated from a copper one, it will be positive, consequently leave the copper negative if insulated: whether this experiment be performed as I have therein mentioned, or according to Mr. Wilson, the one reduced into filings, and afterwards sifted through holes made in the other metal, the electricity excited must fall under the same denomination, the difference will be that the quantity of electricity excited will be more by Mr. W.'s method than mine, because he has multiplied the separations, a fact well worth notice.

Mr. W.'s chief object in view seemingly, is to prove that the separation of metals is the cause of the electric fluid being excited and not touching. I cannot perceive that these experiments throw any light upon that subject, as both touching and separating are here employed.

In my second volume on electricity, published in Amsterdam, anno 1782, I have proved by experiment that neither *touching* nor *friction* separately employed excites electric fluid, friction and separating jointly employed, is a powerful exciter of electricity on glass; touching and separating jointly employed on glass excite electric fluid in a slight degree, and only when the state of the atmosphere is favourable.

In the Philosophical Magazine for Nov. 1804, page 120, <sup>Haüy's obs. on metallic substances,</sup> we have Mr. Haüy's observations on the electricity of metallic substances: he does not inform us what shape his *silver* pieces were, or whether they were pure or with alloy; he however <sup>imperfect.</sup> does not hesitate in pronouncing of it to be positively electrified by friction; so that it does not seem that he has entered very wide into the subject, or he would have perceived some remarkable changes to take place in that metal by friction, <sup>Singular effect of rubbing coins, &c.</sup> and particularly in coins. They will change from positive to negative, and *vice versa*, without any visible cause. If a dollar and a half-crown be stuck to the ends of two sticks of sealing-wax, and rubbed separately upon woollen cloth, they will be found, after the friction, sometimes positive and sometimes negative, and sometimes one positive and the other negative, without varying the manner of friction. If pure silver, or silver with different proportions of alloy, be melted down to a button, and used in that shape, or hammered flat, representing coins, they are for the most part positive by friction. These experiments upon metals are not new; I believe they were first begun by Mr. Henly, and inserted in the Philosophical Transactions; but I have not the data at hand, and I do not remember that he had observed the above-mentioned property of this metal.

## SCIENTIFIC NEWS, ACCOUNT OF BOOKS, &c.

### *Note on the New Planet Juno.*

I HAVE not had any late account of the new planet announced at page 301 of our 9th volume (Dec. 1804) in a letter from M. Bode. The discoverer's name, who is Mr. Harding of Lilienthal, near Bremen, was not then mentioned.—For the present I give the following notes from the *Journal de Physique*, Thermidor last.

On the 5th of September, 1804, its right ascension was  $1^{\circ} 52'$ ; declination  $0^{\circ} 11'$  north. M. Burckhardt observed it on the 23d of September at  $359^{\circ} 7'$  and  $4^{\circ} 6'$ ; whence he concludes that the duration of its revolution is five years and a half. Its inclination  $21^{\circ}$ ; excentricity one quarter of its radius; mean distance from the sun three times greater than that of the earth.

Its

Its diameter could not be measured, but it appeared like a star of the eighth magnitude. It seems nearly equal to that of Ceres, or the planet discovered by Piazzi.

*Discovery of Fluoric Acid in the Topaz.\**

Saxon topaz  
analysed by  
Vauquelin.  
Brazilian by  
Descotils.

IN the year 1797, M. Vauquelin analysed the Saxon topaz, and found its constituent parts to be, silex 31, alumine 68. Mr. Descotils soon after examined the Brazilian topaz; but as there was a loss of 18 per cent. in his first analysis, and 12 in his second, he did not think fit to publish the results of his labours at that time; and other circumstances prevented him from pursuing the inquiry, as he had intended.

Klaproth discovered fluoric acid in the Saxon topaz.

Not long ago Mr. Klaproth wrote to Mr. Hany, that he had found fluoric acid in the Saxon topaz. Mr. Laugier made several experiments to verify this discovery, but without success. It is true in the analysis with potash he found a deficiency of 16 per cent. but though he reduced the topaz to an impalpable powder, and did all he could to expel the fluoric acid from it by means of the sulphuric, he was unsuccessful. Mr. Vauquelin on his return applied himself to the same research, and we here present the result of his labours. Not knowing what process Mr. Klaproth had employed, he tried that which seemed to him most likely to succeed. He first heated the topaz with caustic potash in a silver crucible in the usual way. After he had diluted the mass with water, he introduced it into a retort, and poured on it sulphuric acid. White fumes soon arose, and these, being collected, exhibited all the characteristics of fluoric acid combined with silex. The latter came almost wholly from the stone, as the retort was not perceptibly attacked by the acid.

Sulphuric acid incapable of expelling it.

Vauquelin treating the topaz with potash and sulphuric acid expelled the fluoric.

The Brazilian topaz gives the same result.

The same experiment with the Brazilian topaz gave the same result; and there is every reason to believe, that the Siberian, which Mr. Vauquelin is now analysing, will afford the same products. Thus we may now consider this gem as a siliceous compound, consisting of fluoric acid, alumine, and silex, or a true alumino-siliceous fluuate; and the discovery must be considered as of the highest importance in mineralogy.

Topaz a true alumino siliceous fluuate.

\* Bulletin des Sciences, No. XC. p. 282, Sept. 1804.



Mr. Vauquelin next inquired what might be the circumstance, that led him into an error when he first analysed the topaz; and he imagines, that it was his treating the alkaline mass with muriatic acid, instead of employing the sulphuric; and that probably not heating it to a sufficient degree to expel the fluoric acid, from fear of decomposing the muriate of alumine, he precipitated the fluoric acid combined with the alumine, when he added ammonia to separate the alumine from its muriatic solution.

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*Examination of Crude Platina, by Messrs. TENNANT and WOLLASTON.\**

THE editors of the *Annales de Museum* observe, that platina, when taken from the mine, appears from the experiments of Descotils, Fourcroy, and Vauquelin, to be mixed with iron, chrome, and other metals, among which may be one or two that are new. Mr. Tennant has given names to two, and Dr. Wollaston to two others; but the French journalists congratulate their countrymen for being less hasty, and waiting till they are assured these new metals really exist.

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*Prize Questions in France.*

The Society of Agriculture and the Arts, in the department of the North, proposes for the subjects of two prizes, which shall be adjudged, the first in the first of Fructidor, in the year XIII. and the second in the first fortnight of Fructidor, in the year XIV. the two following questions:

*First Question.*—"What method of propagating, rearing, feeding, and housing the sheep of the race now existing in the department of the North, ought to be followed in this department, to obtain wool from these animals, equal in quality to the best wools from English sheep?"

*Second Question.*—"An insect known in the country under the improper denomination of *puceron*, has this and several preceding years attacked and destroyed the greater part of the flowers of the colza.—"What is this insect? Under what generic and specific name have the most celebrated naturalists described it? What

\* Bulletin des Sciences, No. XC. p. 234, Sept. 1804.

is its life, either in the state of coleoptera in which it is found on the colza, or in the state of larva? What natural enemies, what artificial means of destruction can be opposed to it in either state? Generally, what care can preserve the colza from its ravages?"

Each of these two prizes will be a gold medal of the value of 150 francs.

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*Russian Circumnavigation.*

Russian voyage  
round the world.

By a courier, expedited by the governor of Kamschatka, news was received at Petersburg on the 21st of December, that the vessels belonging to the expedition round the world under the orders of M. de Krusenstern, had arrived, on the 26th of July last, at the harbour of Peter and Paul, in Kamschatka, and that, up to this time M. Krusenstern had not lost a single man of his retinue, nor had he any sick in the squadron. In his voyage he had visited the Marquesas and Sandwich Islands. M. Krusenstern purposed sailing for Japan, towards the end of August.

Europeans found  
at the Marquesas,  
who had forgotten  
their own  
language.

At one of the Marquesas, where the vessels stopped a few days, he had taken on board a Frenchman and an Englishman, to bring them back to Europe. Captain Krusenstern says he had not yet been able to discover how these two individuals had come to this island; both of them having completely forgotten their original language. He thought however that he could comprehend that they had arrived there on board an American vessel which had been shipwrecked on the coast. The Frenchman speaks the language of the islanders very well, and has adopted all their customs, habits and manner. There is no doubt that they will soon recover the use of their language, in a daily intercourse with Europeans; and that they will then be able to give an account of their adventures, as well as information respecting the islanders, among whom they have lived so long. At least, this is expected with impatience.

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*Geological Journey from the Academy at Warsaw.*

Journeys of discovery in  
mineralogy and natural  
history.

The Society of the Friends of the Sciences at Warsaw have charged two of its members, M. Carteau and Stacio, to undertake a mineralogical and physical expedition into the Carpathian mountains. Another member of the society has already examined

examined the eastern part of them, with relation to mineralogy, geogony and oryctognosy. At this time he is travelling over the mountains in the interior of Austria, from whence he will proceed to upper Italy, and to the Alps of Switzerland. When this journey is completed, he will undertake another to Caucasus.

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*Lalande's Proposal of a new Scale of the Thermometer.*

M. de Lalande proposes to adopt a thermometer-scale, which shall remedy all the inconveniences of those now in use. His mean point is taken from the natural state of the globe, which he fixes at  $9\frac{1}{2}$  degrees of Reaumur's thermometer, and he takes the 10 millionth part of the volume of mercury for the measure of a degree. Among the advantages of this instrument he reckons a simplification of expression, which will give a facility to comprehending what was before without meaning to the public. For example, the degree of heat of common summers, and the degree of cold of our mean winters, will be both expressed by 30: The degree of 40 will indicate a hot summer and a severe winter, &c.; another advantage will be derived from the smaller interval of the degrees, which will remove the necessity of having recourse to fractions in the greater number of observations. The boiling point of water is  $+132^{\circ}.8$  of the proposed thermometer, and  $-74^{\circ}.4$  is the point of the congelation of mercury. Ice melts at  $-17^{\circ}.9$  and  $-44^{\circ}.2$  is the zero of Fahrenheit.

New scale of thermometer.

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*Two Kinds of Honey.*

In a note to Dr. Delametherie, Proust announces the discovery of two kinds of honey; the one liquid, the other dry, not deliquescent, crystallizable in its manner, and less saccharine than sugar: they are separated by spirit of wine, to which end granulated honey must be operated on.

*J. de Physique.*

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*Palladium.*

Experiments made by Messrs. Rose and Gehlen, and others by Richter, to obtain palladium, are given at length in the Journal of Chemistry, published in German by Klaproth and Richter. Attempts to form palladium with mercury and platina.

These



These philosophers followed Mr. Chenevix's process with great care, but did not produce that metallic body. In the precipitation of muriate of mercury and muriate of platina, they had a black powder, which always afforded the metals separate from each other.

Richter was not more successful. He verified that the green sulphate of iron does not decompose either the muriate of mercury or that of platina. The other facts he observed were to the same effect as those of Rose and Gehlen. He always found the mercury of his precipitate separated from the platina by heat.

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*Traité Élémentaire d'Histoire Naturelle, &c. An elementary Treatise on Natural History, by A. M. CONSTAT DUME'RIE: Composed by Order of Government for the Use of the National Lyceums, 1 Vol. 8vo. Paris\*.*

Elementary works on natural history very defective.

There is not one of the sciences, the elementary works of which have been so long neglected as natural history. Sometimes this title has been given to collections of tales fit only to amuse children, but not calculated to make them acquainted with nature as a whole, and with the progress of the science: at other times authors have entered into discussions too abstruse, or contented themselves with a mere nomenclature, always dry and sterile to beginners, to whom names give no idea of objects with which they are unacquainted. Mr. D. has preserved a just medium between these extremes, while he observes an accurate and methodical arrangement. He makes us acquainted with the whole of the productions of nature, and the method of studying and classing them, choosing for examples in every section such as are most remarkable for their uses or singularity; he continually excites the attention and curiosity of his pupils; and he presents to them a number of facts necessary to be known,

Mr. D. has pursued a better method.

His arrangement proceeds from the most simple things to the more complex.

In this work Mr. D. has adopted an arrangement, the reverse of what is usually employed in books of natural history; that is, he always proceeds from the most simple to the most complex. He begins with unorganized substances, proceeds hence to plants, and lastly to animals; and in these he commences

\* Bulletin des Sciences, No. 90, p. 236, Sept. 1804.

with zoophytes; and ends with man. This arrangement has its advantages, the advantage of instilling ideas gradually into the mind of the scholar, and avoiding a number of repetitions and anticipations. The history of organised substances gives him an idea of bodies unmixed with any other ideas: that of vegetables shows him organization and life in their most simple state: and these he perceives gradually become more complicated as he ascends through the different classes of the animal kingdom, so that the history of each class is but little more than an exposition of the organs and faculties it enjoys beyond those of the preceding.

Though the discussion of any new idea seems contrary to the essence of an elementary work, it is obvious, that such a work cannot be well executed but by a man capable of considering the whole of a science in its proper point of view. In this respect the naturalist will here read with pleasure the article of general observations placed at the head of each part: he will distinguish the history of insects, which Mr. D. has treated after a new plan: and he will notice the chapter on man, in which the author displays the physical characters that distinguish man from brutes, and the consequences respecting his manners that arise from his very structure. This chapter may be considered as the connecting link between physics and metaphysics.

Mr. D. has treated the insects on a new plan.

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*Elemens de l'Art de la Teinture, &c. or Elements of the Art of Dying; with a Description of the Process of Bleaching by the oxygenated Muriatic Acid. Second Edition: by C. L. and A. B. BERTHOLLET. Paris.*

This new edition of a work of the first merit and celebrity is spoken of in the Foreign Journals, as being considerably improved by the former author and his son. The great perspicuity and order which are seen in all the productions of this eminent chemist, and his own unremitted labours in the science, are a sure guarantee to the same effect.

Berthollet on dying.

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*Considerations on Organized Beings. By J. C. DELAMETHERIE.*

The science of natural history is indebted to M. Delametherie for several interesting works which contain ideas of advantage to the progress of human knowledge. Besides the

Delametherie on organized beings.

Journal de Physique, &c. of which he is the editor, he has published a Theory of the Earth; a Treatise on Man; different Physiological Views of the animal and vegetable Kingdoms, on Vital Air, &c. In this new work the author compares the structure of animals and plants, and applies to vegetables the different systems or apparatuses of organs and vital functions, which X. Bichat has proposed in his General Anatomy. M. Delametherie has benefitted by the experiments and labours of several philosophers of merit. All, perhaps, may not adopt some of the opinions he offers, the conquest of minds being still more difficult than that of hearts; but they, at least, deserve examination, and may lead to unexpected results.

*Bibliothèque de Sonini.*

### TO CORRESPONDENTS.

*My best acknowledgements are due to Mr. D. who has favoured this work with a precise statement of the elucidation of Mr. Boswell's second proposition; but he will perceive that the paper of Mr. GOUGH, which was already printed when his letter came to hand, has rendered it unnecessary.*

*I believe the readers of this Journal will think with me, that the dispute between C. L. and Mr. E. WALKER has proceeded at least as far as the interests of science demand. In a letter from C. L. before me, I have supposed the following explanatory sentences to afford no new ground for discussion, and therefore extract them, and hope the business will end here. C. L. says:*

*"I cannot conceive how expressions so plain could be misunderstood, and hope you will permit me to endeavour to make them plainer if possible. I have denied Mr. WALKER's facts; that is, I have denied the truth of his narrative respecting certain supposed facts, and I have pointed out an easy way of convincing me and the world that he is not deceived; namely, that you should repeat his experiments, and see if what he asserts be true. But Mr. WALKER declines examining the remainder of my letter, which is an indistinct, though sufficiently clear way of saying, that he does not choose to risk his supposed facts by putting them to such a trial."*



Mr. Dodd's Gun Lock.

Fig. 3.

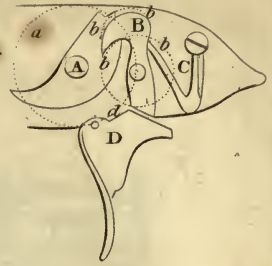


Fig. 1.

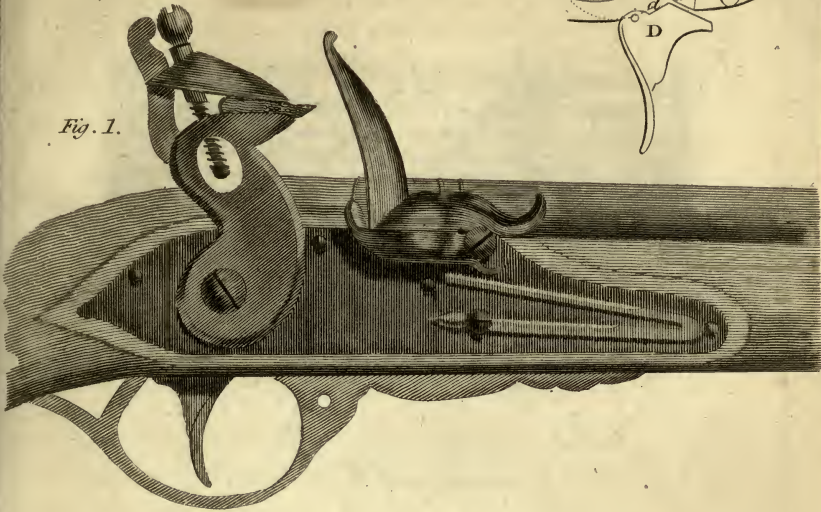


Fig. 4.

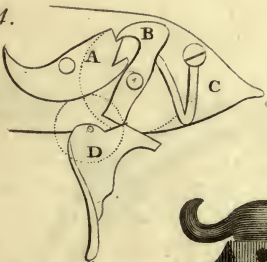


Fig. 2.

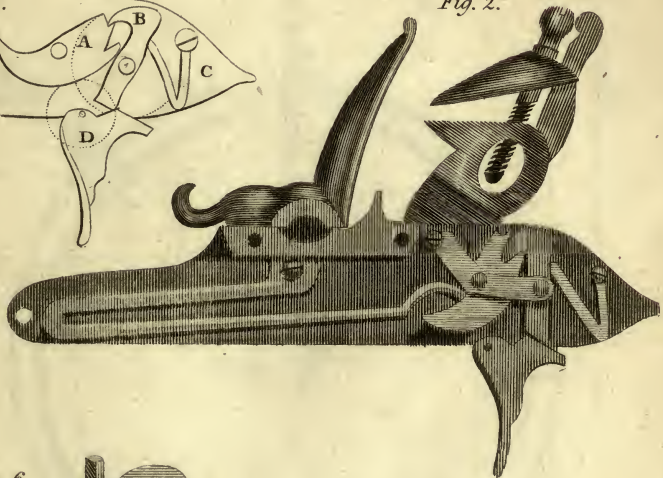


Fig. 6.

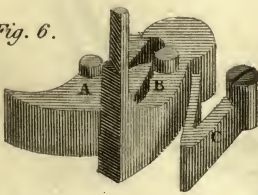
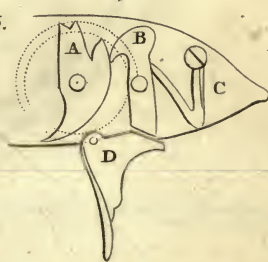


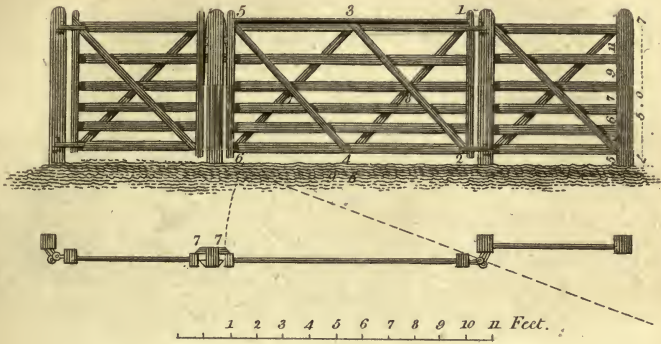
Fig. 5.





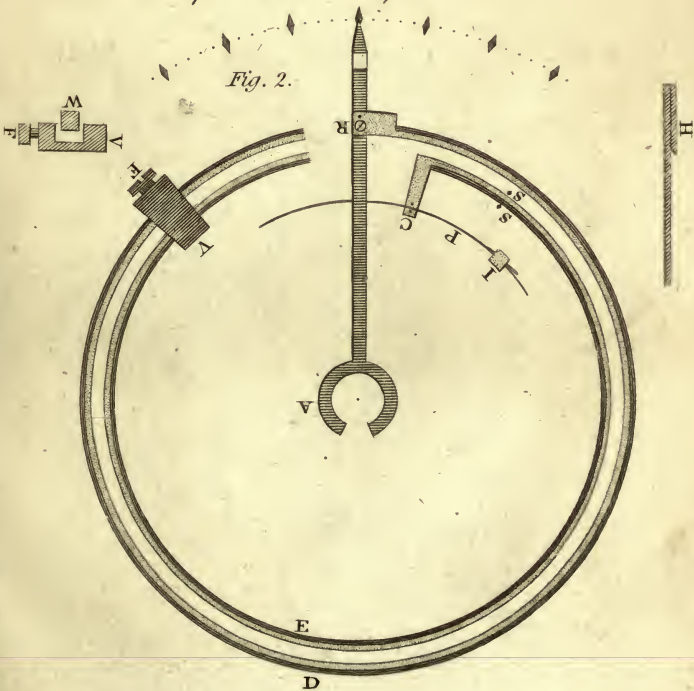
*Mr. Waistell's improved Field Gate.*

Fig. 1.



*Mr. Scott's compensation Curb, for a Time piece.*

Fig. 2.









# Bavarian Method of evaporating Salt Waters.

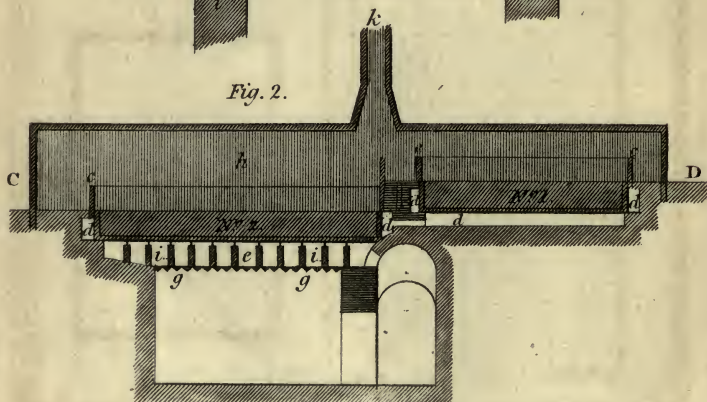
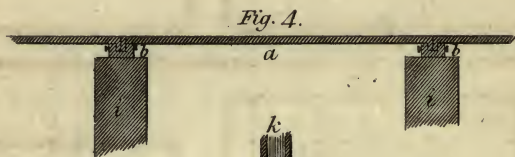
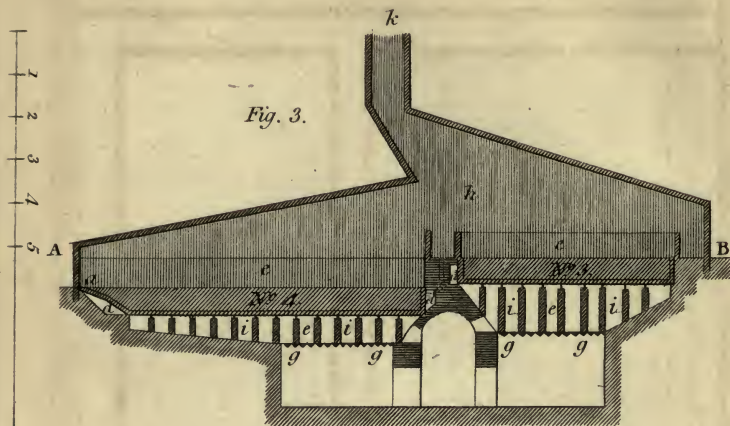
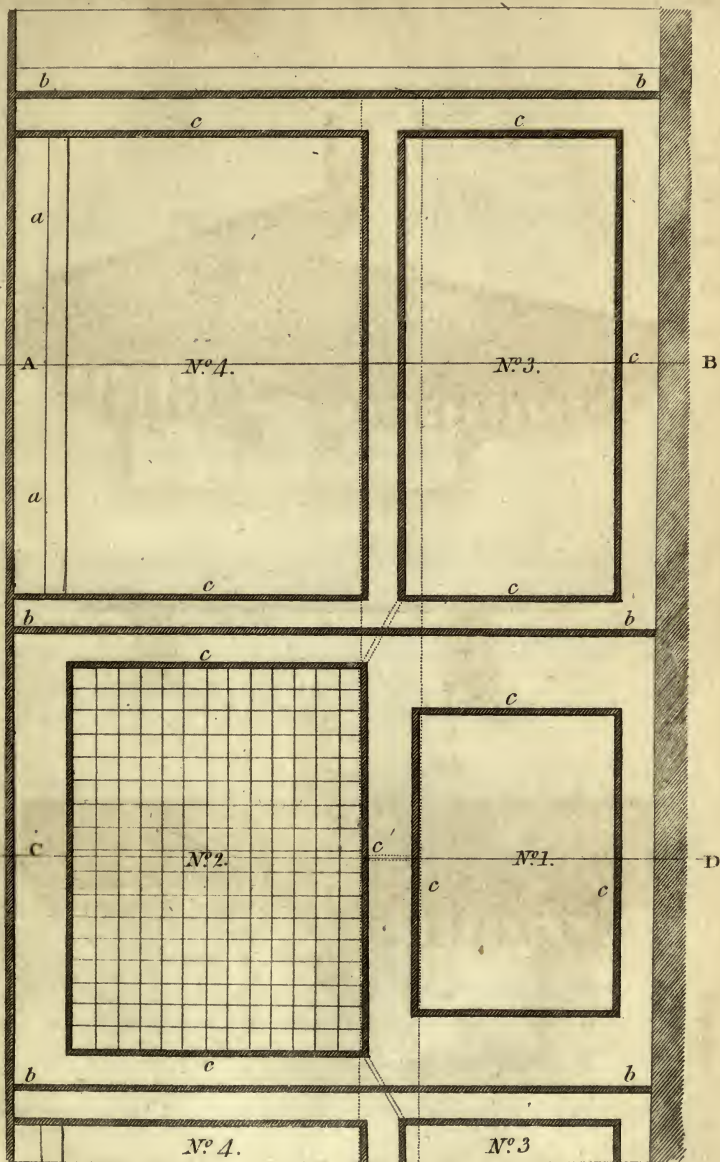
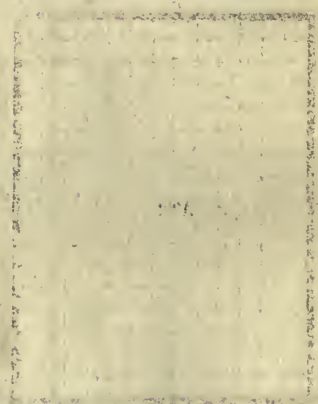
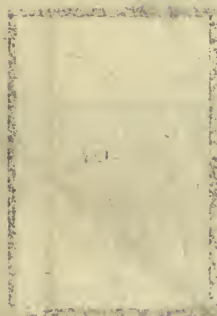
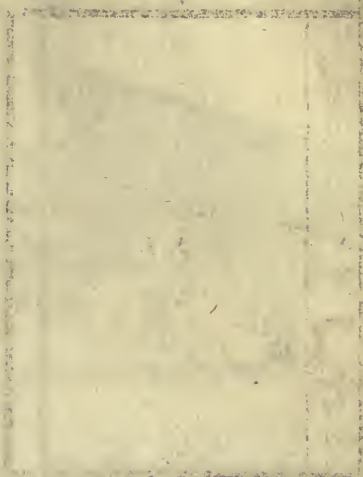
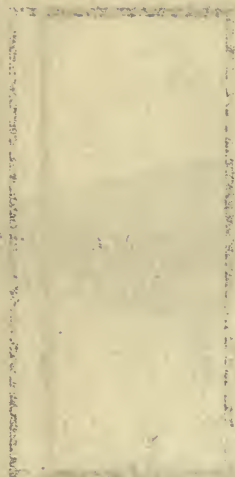




Fig. 1.





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A  
JOURNAL  
OF  
NATURAL PHILOSOPHY, CHEMISTRY,  
AND  
THE ARTS.

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JUNE, 1805.

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ARTICLE I.

*Experiments on the Gases obtained by the destructive Distillation of Wood, Peat, Pit-Coal, Oil, Wax, &c. with a View to the Theory of their Combustion, when employed as Sources of artificial Light; and including Observations on Hydro-Carburets in general, and the Carbonic Oxide: By Mr. WILLIAM HENRY. (Communicated by the Author.)*

THE gas obtained by the destructive distillation of pit-coal, has become an object of considerable interest and importance, in consequence of its successful application (by Mr. Murdoch, of Soho, near Birmingham \*) to the purpose of affording light. Having constructed an Argand's lamp last winter, with the view of effecting the combustion of the gas on Mr. Murdoch's plan, I made previous trials with pure hydrogen gas, with carburetted hydrogen obtained by passing water over ignited charcoal, and with the carbonic oxide; but found that each of them burned with so trifling a production of light, as to be altogether unfit for the purpose of illumination; while the light evolved by the gas from coal was little, if at all, inferior to

Process of Mr. Murdoch to illuminate by gas from coal.

Neither hydrogen nor carbonic oxide give any notable light; but the gas from coal is nearly equal to oil.

\* See the statement of Mr. M's claim to the discovery in the postscript.



This last gas burns with much (though diminished) light after it has deposited its condensable matter.

Products by burning the gas from coal with oxygen and also other gases.

that from good spermaceti oil. So essential a difference in the combustion of these gases, induced me at first to believe, that the gas from coal owes its illuminating property to something mechanically suspended in it; but I was afterwards satisfied, that though it contains, when recently prepared, much that is subsequently deposited, yet that its quality of burning with a bright and compact flame belongs to it, though certainly with considerable diminution, as a permanent gas, after the separation of all condensable matter. It appeared, therefore, worthy of investigation to determine, on what the superior fitness of the gas from coal, for evolving light, depends; and to connect the theory of its combustion with that of other substances, commonly employed as sources of artificial light. With this view, numerous comparative experiments were made on the rapid combustion of this gas with oxygenous gas in close vessels, and also on that of other inflammable gases; and their composition may be inferred from the products of these experiments, the principal results of which are contained in the following table :

Table of results.	Kind of Gas.	Oxygen Gas required to saturate 100 Measures.		Carbonic Acid produced.
	Pure hydrogen,	50 to 54	-	-
	Gas from moist charcoal,	60	-	35
	— wood (oak).	54	-	33
	— dried peat,	68	-	43
	— coal, or cannel,	170	-	100
	— lamp-oil,	190	-	124
	— wax,	220	-	137
	Pure olefiant gas,	284	-	179

If the measure of carbonic acid produced, be deducted from the whole oxygen employed, the remainder will express the measure of oxygen which was employed in burning the hydrogen of the gas, and this last doubled will give the

Now if it be assumed (which I believe is as nearly as possible the fact) that in the formation of each measure of carbonic acid, in the above experiments, an equal volume of oxygen gas is employed\*, we shall learn, by deducting the numbers in the

\* Mr. Cruickshank takes it for granted, as the basis of his calculations, that in forming six measures of carbonic acid, seven measures of oxygenous gas are employed. This proportion I believe to be over-estimated. Dr. Priestley observes, (on Air, 2d Edition, III. 377), "I heated  $8\frac{1}{4}$  grains of perfect charcoal in 70 ounce measures of dephlogisticated air, when it still remained 70 oz.

the third column from the corresponding one in the second, what proportion of the consumed oxygen has been allotted to the saturation of the hydrogen of each hydro-carburet. Thus, for example, in the combustion of the gas from coal, 70 parts of oxygen have disappeared, besides that which has entered into the carbonic acid; and, since each measure of oxygen saturates two of hydrogen gas, the gas from coal must contain, in 100 measures, a quantity of hydrogen which, expanded to its usual elasticity, would occupy 140 measures. By a similar mode of estimation, the quantity of hydrogen in other species of inflammable gas may be ascertained; viz. by subtracting the number in the third from the corresponding one in the second column, in each instance, and doubling the remainder.

The above experiments sufficiently explain why the gas from coal evolves so much more light, during combustion, than either hydrogen or the hydro-carburet from moist charcoal, because, in an equal volume, it includes, in its composition, above thrice the quantity of inflammable matter present in hydrogen gas, and nearly thrice as much as is contained in the gas from moist charcoal. The appreciation of the degree of combustibility of each gas, by the quantity of oxygen required for its saturation, entirely agrees, as might naturally be expected, with that founded on the phenomena of silent combustion in an Argand's lamp; for each gas seemed to me to evolve light, as nearly as could be judged, in proportion to the quantity of oxygen consumed by its detonation in a close vessel. Above all others, the olefiant gas\* is decidedly entitled to rank, by the splendor and beauty of the light which it yields; and the violence of its detonation, when fired with a mixture of oxygen gas, also surpasses that of every other inflammable gas. By exploding only .03 of a cubic inch with .17 of oxygen gas, a strong glass tube was

measure of hydrogen originally present.

These facts explain the greater illumination from the coal gas. The light is greater the more oxygen is required for the combustion.

m.; but, after washing in water, was reduced to 40 oz. m." In this experiment one grain and  $\frac{1}{4}$  of charcoal was consumed, and 30 oz. m. of carbonic acid were generated, without any change in the volume of the oxygenous gas.

\* A full abstract of the memoir of Messrs. Deiman, &c. on this interesting gas, may be seen in the 1st Vol. of the 4th Series of this Journal. Its characteristic property is that of being rapidly condensed into oil, by contact with oxygenized muriatic acid gas.

Specific gravity of an inflammable gas (freed from carbonic acid) is a test of its fitness to afford light.

It is probable that the inflammable gases are mixtures of few simple gases.

Gas from coal appears to be hydro-carburet with perhaps some carbonic oxide.

Gas from ignited charcoal and water is probably carbonic oxide with hydrogen and a little hydro-carburet.

shattered with violence; and a Volta's eudiometer,  $\frac{1}{4}$  of an inch in thickness was burst by less than a cubic inch of a mixture of the two gases. The specific gravity of the inflammable gases, when perfectly freed from carbonic acid, is another competent test of their fitness as sources of light. Thus the specific gravity of the gas from moist charcoal, (common air being 1000) according to Cruickshank is 480; of the hydro-carburet from alcohol 520, and of the olefant gas, as determined by the Dutch chemists, 909.

From the limitation to the proportions, in which bodies in general, having a susceptibility of chemical union, are capable of combining, it seems to me reasonable to infer, that carbon and hydrogen do not unite in all possible proportions, forming so many distinct compounds; but that the various inflammable gases are mixtures of a very few simple ones. Of those at present known, pure hydrogen gas; the carburetted hydrogen, which by combustion affords an equal bulk of carbonic acid, and consumes twice its bulk of oxygen; the carbonic oxide; and the olefant gas, it will appear, may be traced into the mixed gases comprehended in the foregoing table. The gas from coal I apprehend to be principally hydro-carburet, with perhaps some portion of carbonic oxide; the presence of which last is rendered probable, because the gas from coal is saturated by less than twice its bulk of oxygen, though it gives an equal volume of carbonic acid. Now the gas from marshes, which, with Mr. Cruickshank, Mr. Dalton finds to be hydro-carburet, contaminated with about 20 *per cent.* azotic gas, consumes, making allowance for this adulteration, double its volume of oxygen gas; and since the gas from coal requires a less proportion than this of oxygen, and yet gives an equal product of carbonic acid with that from marshes, it is fair to presume, that it must previously have contained some oxygen, which, after washing it with lime-water, can subsist in no other state than that of the carbonic oxide.

The gas obtained by decomposing water over ignited charcoal, is most probably a mixture of carbonic oxide with hydrogen gas, and perhaps a little hydro-carburet. On no other presumption can the results of its combustion be explained; since the quantity of oxygen required in saturating 100 measures is only ten more than are consumed by 100 measures of pure hydrogen, though 35 m. of carbonic acid, containing at least



35 of oxygen, are found after combustion. Now, according to Mr. Cruickshank, these 35 m. of carbonic acid, if formed from carbonic oxide, would require only 15 additional measures of oxygen gas, which is not very remote from the truth.

Reasoning in the same mode, the gases from wood and from peat will appear to be mixtures in different proportions of two at least of the above-mentioned, viz. hydrogen, and the carbonic oxide. The results of the combustion of the gases from wood and peat evince that they differ considerably from that obtained from coal; and contain much less uncombined inflammable matter. Another circumstance of distinction also is, that, before being washed with lime-liquor, the gas recently prepared from wood or from peat, contains from  $\frac{1}{4}$  to  $\frac{1}{3}$  its bulk of carbonic acid; whereas the gas from coal loses by this absorption only from  $\frac{1}{27}$  to  $\frac{1}{13}$ . \* In my first experiment, I found a large admixture of azote in all these gases; but this afterwards proved to be accidental and not essential; since, by careful distillation in glass retorts, of the substances that afforded them; the gases were obtained entirely free from this contamination.

The gases obtained by the destructive distillation of oil and of wax, it may be observed in the table, consume considerably more oxygen than the gas from coal. This circumstance first led me to suspect that they might possibly be mixtures of the olefiant gas with carburetted hydrogen; and on applying the oxygenized muriatic acid gas, this suspicion was fully verified. One measure of the gas from oil with one of the oxygenized gas, were reduced speedily to  $1\frac{3}{4}$ ; a like diminution was produced in the gas from tallow; and that from wax had its bulk still further contracted, only  $1\frac{1}{2}$  m. being left by similar proportions.

Gases from wood and from peat contain less of uncombined inflammable matter than the coal gas.

The gases from oil and from wax are pure hydro-carburets, except that the first contains one-eighth and the latter one-fourth of olefiant gas.

\* The condensible products also of coal probably differ from those of wood and peat. If an intermediate vessel be placed for their reception, it emits, after the distillation, a strong smell of ammonia. This was long ago observed by Lord Dundonald, who enumerates, among other products of coal, the volatile alkali (see a pamphlet "on the Uses and Qualities of Coal Tar," published by his Lordship in 1785.) This production of ammonia I have not observed from peat or wood; nor do I find it mentioned in a History of Peat, including the results of its distillation, &c. published in the second vol. of the Edinburgh "Essays Physical and Literary."

In order to ascertain how much of the diminution was owing to the condensation of olefiant gas, the proportion of oxygenized acid, required for the saturation of that gas, was carefully ascertained. After several trials, it was found that 3 m. of the oxygenized acid with  $2\frac{1}{2}$  pure olefiant gas, prepared according to the Dutch chemists, left only 0.15 m. of common air derived from the vessels. It appears, therefore, that the gas from oil and from tallow contains about  $\frac{1}{8}$ , and that from wax  $\frac{1}{4}$  olefiant gas, the rest being pure hydro-carburet.

Hence it is seen why washing diminishes the product of carbonic acid afforded by burning an hydro-carburet. For it takes away the olefiant gas.

The results of these experiments, in connection with a fact communicated to me by Mr. Dalton, explain some circumstances observed by Mr. Cruickshank, for which that ingenious chemist was at a loss to account, viz. the great variation in the products of carbonic acid, obtained by burning the same hydrocarburet when washed and when unwashed, or when long kept in contact with water; though the gas, when originally procured, was perfectly free from carbonic acid. The olefiant gas, Mr. Dalton has ascertained, is far more absorbable by water than other species of hydrocarburet, viz. in the proportion nearly of  $\frac{1}{3}$ . Now the gas from camphor, I find to contain much olefiant gas, and indeed this might have been inferred from Mr. Cruickshank's own statement, who observes that this gas, by admixture with oxygenized muriatic acid, undergoes a considerable diminution of bulk. The pure hydrocarburet, on the contrary, I have never seen at all condensed by contact with this gas, in the rapid manner observable in olefiant gas or mixtures containing it; though, by confinement together for some hours, they are mutually decomposed into common muriatic acid, carbonic acid, and water. The hydrocarburets from ether and alcohol also contain olefiant gas; and this, I apprehend, will be found to be the fact with all inflammable gases, which by combustion give more than their own bulk of carbonic acid. The variable products of carbonic acid, obtained in Mr. Cruickshank's experiments, from equal quantities of different hydrocarburets, cannot, therefore, be considered as denoting so many distinct species of carburetted hydrogen; but as owing to the admixture with this of various proportions of olefiant gas.

Hydrocarburets from ether and alcohol also contain ol. gas; which is the general cause of the difference in carb. acid products by Mr. Cruickshank.

Error of that author respecting the constitution of carburetted hydrogen gases.

It will not, I am persuaded, be regarded as indicating a wish to detract, in the smallest degree, from the credit due to Mr. Cruickshank, whose memoirs on the hydrocarburets and carbonic



carbonic oxide I estimate among the most ingenious, and generally speaking, the most satisfactory examples of chemical research, if I observe that the part of his table (vol. V. p. 8 of the 4th series of this Journal) which relates to the constitution of the carburetted hydrogen gases, I consider as entirely erroneous. To excite strong suspicion of the accuracy of the proportions assigned to these gases, it is surely sufficient that one of them (the gas from moist charcoal) is stated to contain in 100 cub. in.  $= 14\frac{1}{2}$  grains, no less than 9 grains of water, a proportion absolutely inconceivable; and the same objection applies, in a less degree, to the other cases. Now in 100 cubic inches of the muriatic acid gas, I found the absolute quantity of combined water to be only 1.4 gr. (Phil. Trans. 1800); and it is rendered highly probable, by the experiments of Clément and Deformé (*Ann. de Chim.* XLII. 121) that all gases contain the same quantity of water. In the instance of the gas from charcoal, Mr. C. was most probably misled, by not having suspected the presence of the carbonic oxide; and the correction is to be made as follows. One hundred cubic inches ( $= 14\frac{1}{2}$  grains) combined with the proper quantity of oxygen, gave 19 grains of carbonic acid, containing very nearly 4 grains of carbon; and supposing the carbon in the gas before combustion to have been in the state of carbonic oxide, it would be combined with about 9 grains of oxygen, and would constitute 13 gr. or  $43\frac{1}{2}$  cub. in. of carbonic oxide.— There remains then only  $1\frac{1}{2}$  grain, of the  $14\frac{1}{2}$  submitted to experiment to be accounted for, which is very exactly made up by the residuary  $57\frac{1}{2}$  cub. in. of hydrogen gas, taking the weight of 100 cub. in. of hydrogen to be 2.6 gr. The water contained in the gas may, I think, be set out of the question; for it must be recollected that the product of the combustion is in part aeriform; and it may be considered as a tolerable approximation to the truth, that the gas from charcoal contains, in 100 inches, 43 of carbonic oxide, the remainder being principally hydrogen gas.

With respect to the presence of hydrogen in the carbonic oxide, which has been a topic of controversy, neither the fact nor the negative can, I think, be at present with certainty affirmed. If however any hydrogen be contained in it, I should deem it an accidental and not an essential ingredient, and am of opinion that, if present at all, it exists in the state of hydrogen

Carbonic oxide  
does not contain  
combined hydro-  
gen, &c.



gen gas; for I find that the carbonic oxide is not expanded by electrical discharges, which would assuredly happen if the carburetted hydrogen were one of its constituents, or accidentally mixed with it.

Elucidation of  
the theory of  
lamps, &c.

The oil, &c. is  
decomposed in  
the wick,

—into olefiant  
and carburetted  
hydrogen gases,  
which are then  
burned. Whence  
the fitness of any  
fuel to give lu-  
minous flame  
may be known  
from its destruc-  
tive distilla-  
tion,

The gases from  
coal, &c. though  
they offered no  
olefiant gas,  
afford much  
light by an in-  
flammable  
vapour when  
recent.

To return to the theory of lamps, &c. it is proved by the preceding experiments, that the substances ordinarily employed as sources of artificial light, viz. oil, tallow, and wax, afford when submitted to an increased temperature, much olefiant gas; and it has been justly observed by the editor of this Journal (4to series I. 71) "that the wick of a lamp or candle surrounded by flame is exactly in the situation of a body exposed to destructive distillation in a close vessel." In this case the series of capillary tubes composing the wick, serve perhaps precisely the same office as a tube horizontally disposed in a heated furnace, through which an inflammable liquid is transmitted. The fuel previously melted, is drawn up into these ignited capillary tubes, and there resolved into olefiant and carburetted hydrogen gases, from the combustion of which gases, and not merely of a condensible vapour, it appears to me that the illumination chiefly proceeds. Hence it is not improbable, that the proportion of olefiant gas and hydrocarbon, obtained by the distillation of any substance, will be a tolerable measure of its fitness for affording light. In distillations of this kind, however, the degree of heat is of considerable moment, for I have found that the olefiant gas may be obtained or not, at pleasure, during the decomposition of ether, alcohol, oil, &c. by varying the temperature to which the containing vessels are exposed.

In the gases from coal, peat, and wood, though these substances yield no olefiant gas, the defect is compensated by an inflammable vapour diffused through them when recent, and which is even not removed by passing through a small quantity of water. Gas from coal, however, which had stood over water upwards of a month, I have found burns with considerably impaired brilliancy, though still with a far more dense and bright flame than hydrogen gas, or the gas from charcoal.

*Manchester, May 4, 1805.*

## POSTSCRIPT.

Since the preceding pages were written, I have examined a fresh portion of the gas from coal, obtained by very cautious distillation, with a view to ascertain whether any olefiant gas can be procured from that substance. Of this gas five measures mixed with five of oxygenized muriatic acid, were reduced to nine; from which it should appear that the gas from coal may possibly contain  $\frac{1}{20}$  of olefiant gas. The production of oil, however, was not so manifest as in other instances; and I judged it to have happened chiefly because an iridescent film was visible on the surface of the water when held between the eye and the light.

I am enabled also, by a letter received this morning from a friend who is well acquainted with the progress of Mr. Murdoch's experiments, in answer to some queries from me, to state specifically the grounds of that gentleman's claim to the important application of coal as a source of artificial light.--- This I cannot do better than by an extract from the letter.

"In the year 1792, at which time Mr. Murdoch resided at Redruth in Cornwall, as Boulton and Watts principal agent and manager of engines in that county, he commenced a series of experiments upon the quantity and quality of the gases contained in different substances. In the course of these, he remarked, that the gas obtained by distillation from coal, peat, wood, and other inflammable substances, burnt with great brilliancy upon being set fire to; and it occurred to him, that by confining and conducting it through tubes, it might be employed as an economical substitute for lamps and candles. The distillation was performed in iron retorts, and the gas conducted through tinned iron and copper tubes, to the distance of 70 feet. At this termination, as well as at intermediate points, the gas was set fire to, as it passed through apertures of different diameters and forms, purposely varied with a view of ascertaining which would answer best. In some, the gas issued through a number of small holes, like the head of a watering pan; in others it was thrown out in thin long sheets, and again in others in circular ones, upon the principle of Argand's lamp. Bags of leather and of varnished silk, bladders, and vessels of tinned iron were filled with the gas, which was set fire to, and carried about from room to room, with a view of ascer-

The gas from coal appears by exp. with ox. m. acid gas to contain a small portion of ol. gas.

History of Mr. Murdoch's experiments for giving light by gas from pit-coal.

History of Mr. Murdoch's experiments for giving light by gas from pit coal.

ascertaining how far it could be made to answer the purpose of a moveable or transferable light. Trials were likewise made of the different quantities and qualities of gas produced by coals of various descriptions, such as the Swansea, Haverford-west, Newcastle, Shropshire, Staffordshire, and some kinds of Scotch coals."

"Mr. Murdoch's constant occupations prevented his giving farther attention to the subject at that time; but he again availed himself of a moment of leisure to repeat his experiments upon coal and peat, at Old Cumnock in Ayrshire in 1797; and it may be proper to notice that both these, and the former ones, were exhibited to numerous spectators, who, if necessary, can attest them. In 1798, he constructed an apparatus at Soho Foundry, which was applied during many successive nights, to the lighting of the building; when the experiments upon different apertures were repeated and extended upon a large scale. Various methods were also practised of washing and purifying the air, to get rid of the smoke and smell. These experiments were continued, with occasional interruptions, until the epoch of the peace in the spring of 1802, when the illumination of the Soho manufactory afforded an opportunity of making a public display of the new lights; and they were made to constitute a principal feature in that exhibition. I do not know exactly at what time the first trials were made, or published in France. The first notice we received of them here, was in a letter from a friend at Paris, dated the 8th of Nov. 1801, in which he desires me to inform Mr. Murdoch, that a person had lighted up his house and gardens with the gas obtained from wood and coal, and had it in contemplation to light up the city of Paris."

"After mentioning the above, I think it is proper to state also, that in the ovens constructed upon Lord Dundonald's plan, at Calcetts in Shropshire, for the purpose of saving the tar, &c. which escapes during the coaking of coal, it has been usual for a number of years past to set fire to the large current of gas as it flies off, and thus procure a bright illumination. This however was not known to Mr. Murdoch, and was never seen by him."



## II.

*Experiments on the Analysis of Goulard's Extract, or the Aqua Lithargyri acetati.* By JOHN BOSTOCK, M. D. Communicated by the Author.

To Mr. NICHOLSON.

DEAR SIR.

I HAVE the pleasure to transmit to you some experiments on the analysis of Goulard's extract, which I hope you may think not unworthy of a place in your Journal.

I am, Sir,

Your obedient Servant,

JOHN BOSTOCK.

Liverpool,

May 5, 1805.

During the course of some experiments on the analysis of animal fluids, I was led to observe the effect of the *aqua lithargyri acetati*, or the extract of Goulard, as a coagulator of mucus, and particularly to notice the superiority of its power over that of the acetate of lead. From these circumstances I was induced to examine into the opinions that had been entertained respecting its composition, but was not able to obtain any satisfactory information. Although it is a compound so well known, and so frequently employed, it appears never to have been made the subject of chemical analysis. In Dr. Thomson's System of Chemistry it is not distinguished from the common acetate of lead; \* Dr. Murray informs us, that "it is merely a solution of acetate of lead in water, with an excess of acid; †" and Dr. Duncan, Jun. conceives, that it does not differ from a solution of the acetate of lead of the same strength. ‡ We meet with nothing specific respecting its constitution in M. Fourcroy's "*Systeme*", § nor is there any light thrown upon it by his acute commentator Proust. || In this dearth of information, I proceeded to make the following experiments.

The extract of Goulard coagulates mucilage more than the acetate of lead.

It has not been examined.

\* Chem. III. 52. † Mat. Med. II. 223. ‡ Edin. Disp. p. 506 § VIII. 203. || Journ. Phys. LVI. 207.

To

Experiments on its composition. The common acetate of lead was dissolved in water.

1. To 200 grains of distilled water were added 60 grains of acetate of lead, in its usual crystalline state; the fluid was kept for about an hour at the boiling temperature, and was afterwards filtered. The residue, when dried, did not amount to more than 2 grains; this was boiled in a fresh quantity of water, when about half of it was dissolved, but one grain still remained not acted upon by the water. It appeared, therefore, that a saturated solution of the acetate of lead was formed; it was transparent and colourless; it slightly reddened paper stained with litmus.

This was precipitated by carbonate of potash. It gave eleven parts carbonate of lead.

2. A solution of the carbonate of potash was prepared, in the proportion of 11.25 grains of potash to 100 grains of water. To 40 grains of the solution of the acetate of lead from No. 1, a quantity of this solution of potash was gradually added; a copious precipitate of the carbonate of lead was produced; after the addition of 60 grains of the alkaline solution there was no farther precipitation, and the fluid slightly affected a paper soaked in the infusion of the mallow flower. The precipitate was carefully collected, and being dried by a gentle heat, afforded eight grains of carbonate of lead.

Goulard's extract gave eleven parts carbonate of lead.

3. Forty grains of *aqua lithargyri acetati* were treated in the same manner with the carbonate of potash; the precipitate formed appeared more copious than in the former experiment, and after the addition of 40 grains only of the alkali, no farther effect seemed to be produced; the fluid affected the mallow paper in the same degree as in the former experiment. The precipitate, being collected and dried, weighed 11 grains.

It did not redden litmus paper. Blue precipitate with litmus infusion.

4. The *aqua lithargyri acetati* did not in any degree redden litmus paper; a few drops of it were added to half an ounce of the infusion of litmus; a precipitate of a beautiful light blue was immediately formed, while the fluid was left transparent and nearly colourless.

The solution of acetate gave 4 gr. brittle clear residue by evap.

5. Twenty grains of the solution of acetate of lead, No. 1, were slowly evaporated; the fluid became extremely viscid, and at length, in some degree, brittle and transparent, and assumed the appearance of dried gum. It weighed about four grains.

Aqua lith. acet. gave 5 gr. white opaque residue.

6. Twenty grains of the *aqua lithargyri acetati* were evaporated in the same manner; it became white and opaque, and when the process was completed, it exhibited the appearance of a number of scales of a pearl colour. It weighed a little more than five grains.

7. A solution of gum Arabic was formed, in the proportion of one part of gum to 100 parts of water. One grain of this solution was added to 39 grains of water, so that the gum only constituted  $\frac{1}{40}$  part of the solution; a single grain of Goulard dropped into it produced a perceptible opacity. Gum precipitated by Goulard's extract.

8. Twenty grains of the saturated solution of the acetate of lead, had one grain of the solution of gum added; the effect was barely visible, certainly less than in the former experiment. —but scarcely by acetate of lead.

I am far from considering these experiments as sufficient to afford a complete investigation of the subject; but I think they may enable us to make some advances towards the truth.

The 40 grains of the solution No. 1. contain 11.6 grains of acetate of lead; by the addition of the alkali, this was converted into eight grains of the carbonate of lead. These eight grains of carbonate consist of 6.72 grains of the yellow oxide, and 1.28 grains of carbonic acid.\* The 6.72 grains of yellow oxide consist of 6.12 grains of pure lead, and .6 grains of oxygen, † so that the 40 grains of the saturated solution contain a little more than 6 grains of pure lead. Forty grains acetate contain six grains metallic lead.

By employing the same reasoning to the analogous experiment with Goulard, we may conclude, that the 11 grains of carbonate produced in this case, consist of 9.24 grains of the yellow oxide of lead, and 1.76 grains of carbonic acid; the 9.24 grains of oxide will be composed of 8.4 grains of pure lead, and .84 grains of oxygen, so that the 40 grains of the *aqua lithargyri acetati* contain nearly  $8\frac{1}{2}$  grains of pure lead. Forty grains Goulard contain  $8\frac{1}{2}$  of lead.

We shall not find it so easy to ascertain precisely the quantity of acetic acid which enters into the composition of the acetate of lead, and the *aqua lithargyri acetati* respectively; but if we trust to the experiments of M. Thenard ‡, we must conclude that 11.6 grains of acetate of lead, contain about three grains of the acetic acid. The quantity of acid in the *aq. lith. acet.* is less than that in the acetate of lead, in the proportion of 40 to 60; therefore the 40 grains of *aq. lith. acet.* will only contain two grains of acid. Deduction of the respective quantities of acid.

Hence it follows, that 40 grains of the solution of acetate of lead consist of, Component parts of each.

\* Thomson's Chem. III. 50.      † Proust, Journ. de Phys.  
LVI. 296.      ‡ Nich. Journ. VI. 223.



	Grs.		Grs.
Lead - -	6.12	The same quantity of Goulard will consist of - -	Lead 8.4
Acetic Acid	3.		Acid 2.
Oxygene	.6		Oxygene .84
Water	30.28		Water 28.76
	<hr/> 40.00		<hr/> 40.00

converting these proportions into quantities of 100 grains each they will be as follows,

	Sol. acet. lead.	Aq. lith. acet.
Lead - - -	15.3	21.
Acetic acid -	7.5	5.
Oxygene -	1.5	2.1
Water - - -	75.7	71.9
	<hr/> 100.0	<hr/> 100.0

The experi-  
ments shew that  
Goulard and the  
acetate are differ-  
ent salts.

From this statement it appears, that in the *aq. lith. acet.* the oxide of lead and the acid exist to each other in the proportion of 23 to 5, or of 100 to 21.74, and we find that M. Thenard has described the perfect acetate of lead, as a salt in which the oxide and the acid exist in the proportion of 100 to 21.79. So near a coincidence between these two proportions can scarcely be regarded as the mere effect of accident; but must rather be considered as a proof that the substances operated upon were nearly, if not altogether, identical. Admitting this to be the case, we must conclude that the *aqua lithargyri acetati* is a saturated solution of the proper acetate of lead, that it is an essentially different salt from the super-acetate of lead, and that it is not, as has been imagined, an accidental compound, but an exactly neutralized salt, the constituents of which exist in a constant ratio to each other.

The former  
being at satura-  
tion,

—and the latter  
a super-acetate.

The neutral  
compound is  
most easily de-  
composed,  
—and is there-  
fore a better test  
of mucus.

It happens in this, as in other instances, that the ingredients composing the completely saturated compound, possess a weaker affinity for each other than when they exist in a different proportion. To this circumstance must be attributed the superior delicacy which Goulard possesses, as a test of animal and vegetable mucus, over the super-acetate of lead, or the common *cerusu acetata*. The *aqua lithargyri acetati* is speedily decomposed by the action of the atmosphere, in consequence of the oxide of lead which enters into its composition having a stronger affinity for carbonic than for acetic acid; this effect takes place in a less degree, in a saturated solution of super-acetate of lead.

From

From the first experiment we learn, that the super-acetate of lead is more soluble in water than is generally imagined; Super-acetate of lead more soluble than generally supposed. Dr. Thomson observes, that it is dissolved only sparingly\*; yet we find that 100 parts of water retain in solution 27 parts of the salt.

## III.

*A concise View of the Theory of Respiration. By*

W. BRANDE', Esq. (*From the Author.*)

THE term respiration implies the reception of atmospheric Respiration, air into the lungs, and its subsequent emission, after having produced changes in the blood necessary to the continuance of life †.

No other gaseous body being capable of producing these changes, it was natural to suppose, that until we became acquainted with the component parts of the atmosphere, very little of the true nature of respiration could be understood. could not be explained till the atmosphere was analysed.

The first great step towards the analysis of the air was made by Dr. Priestley, who in the year 1774 discovered oxygen gas, called by him dephlogisticated air. But we are indebted to Discoveries of Priestley, and Lavoisier. Lavoisier for the most accurate investigation on this subject; who from many experiments, which it is not necessary here to relate, concluded that atmospheric air was composed of oxygen and azot, in the proportion of about 27 parts of the former to 73 of the latter. The air also contains a small quantity of carbonic acid, and a considerable quantity of water (subject however to much variation) is always suspended by it.

Some of the gases are totally unrespirable, that is to say, incapable of being taken into the lungs; for whenever this is attempted, a spasmodic affection of the epiglottis takes place, which by closing on the larynx, shuts up all communication with the organs of respiration. To this class belong all those Gas which cannot be respired, or admitted into the lungs.

\* Thomson's Chemistry, III. 53.

† Respiration has been divided into, 1. Inspiration, or the ingress of air into the cells of the lungs, caused by the enlargement of the cavity of the chest; 2. Into expiration, or the egress of air from the lungs, caused by the contraction of the chest.

Carbonic acid. Its effects; as described by Pilatre de Rozier. gaseous bodies possessed of acid properties. The effects of carbonic acid are described as follows by Pilatre de Rozier:—He went into a brewer's tub which was full of carbonic acid gas; he at first felt a slight heat throughout his whole body, which produced a gentle perspiration; an itching sensation frequently obliged him to close his eyes, and on attempting to breathe, he was prevented by a very violent sense of suffocation. He wished to get out of the tub, but being unable to find the ladder, the necessity of breathing increased, he was seized with a violent giddiness, and felt a tingling sensation in his ears. He at length contrived to extricate himself, and although he then experienced no difficulty in breathing, he was unable to distinguish the objects around him; his hearing was also much impaired. On repeating the experiment he found, that as long as he remained without attempting to breathe, he could readily move or even speak, but whenever he tried inspiration, a violent sense of suffocation came on.

Gases which can be admitted into the lungs. But there are certain gaseous bodies which may be drawn into the lungs, meeting with no opposition from the organs of respiration. Dr. Thomson has divided these into four classes.

The first set, he observes, occasion immediate death, but produce no visible change in the blood; they occasion the animal's death, merely by depriving him of air, in the same manner as were he immersed in water: the only gases belonging to this class are hydrogen and azot. The second set occasion immediate death also, but at the same time produce certain alterations in the blood; and therefore kill, not only by depriving the animal of air, but by certain specific properties:

Gases which kill by a speedy positive action. the gases belonging to this class are, carburetted hydrogen, sulphuretted hydrogen, carbonic oxide, and perhaps also nitrous gas. The third set of gases may be breathed for some time without injury, but death ensues at last, provided their action be long enough continued: to this class belong the nitrous oxide and oxygen gas. The fourth set may be breathed any length of time without injury: the only gaseous body belonging to this class, is the air of the atmosphere, that compound of oxygen and azot every where surrounding the globe.

Nitrous oxide and oxygen kill by a slower action. After an animal has breathed a certain quantity of air for a given time, it becomes totally unfit for respiration; and if the air thus respired be chemically examined, we shall find that atmospheric air loses its oxygen by that process, the oxygen is greatly diminished; and that a considerable quantity of carbonic acid gas has been produced.



It appears from a number of experiments made by Dr. Hales, Dr. Menzies, and Mr. Davy, that the number of respirations made in a given time, as well as the quantity of air taken into the lungs, are liable to considerable variations in different people. Some have calculated the number of respirations at 14 only in a minute, others at 20; Mr. Davy informs us that he makes 26 or 27 in a minute; but having frequently endeavoured to count the respirations made by different people, in a given time, and without their knowledge, I have found them vary from 18 to 26 in a minute, most commonly, however, 20 or 21; and 21 in a minute make 30,240 in 24 hours.

The quantity of air taken in at each respiration, must be in proportion to the size of the person and the capacity of his lungs. About 41 cubic inches of air are taken in at every natural inspiration.

We now come to consider the changes which are produced both in the air and blood, by respiration. 1. On the changes effected in the air. Dr. Priestley, M. Lavoisier, and Mr. Davy, have furnished us with many interesting and instructive experiments on this subject. The changes are, 1. That a portion of the air disappears; 2. That the air expired differs from that first taken into the lungs, in containing carbonic acid, and water in the state of vapour. Dr. Menzies has shewn, that  $\frac{1}{20}$ th of the air inspired disappears in the lungs, and the experiments of Lavoisier, which were made with much precision, differ but little from the above statement. I never knew the quantity of air which disappears to be less than  $\frac{1}{20}$ th part of the whole taken into the lungs; this may however be liable to variation in different people.

It has hitherto been supposed that the portion of air which disappears, consists of the oxygen only: Mr. Davy has, however, given some very strong reasons for supposing that part of the azote likewise disappears during respiration. He supposes that the average quantity of air which is absorbed at every respiration, amounts to 1.4 cubic inches, of which 0.2 are azot and 1.2 oxygen.

Lime-water detects carbonic acid gas in the air emitted from the lungs, and the quantity of this gas may easily be estimated, by receiving the air expired into a graduated glass jar, standing over mercury; a little caustic soda being introduced, the absorption which takes place denotes the quantity

of carbonic acid. Lavoisier has estimated the quantity of this gas thrown out from the lungs in 24 hours, at about 15.5 ounces troy. Mr. Davy makes the quantity thrown out in the same time, amount to 37 ounces, which is about 1 cubic inch at every expiration \*. The quantity however varies from 0.5 cubic inch to 1.5 at different times in the same person, so that

about one cubic inch each expiration.

But this varies.

this accounts for the great variation in the above-mentioned experiments. Moreover the proportion varies, in the same individual, during the 24 hours; for I have found the quantity of carbonic acid gas emitted from my own lungs, to be rather less in the morning than towards the evening; but this also varies in different people.

Aqueous vapour also emitted;

But watery vapour is also emitted in respiration, the greatest part of which is probably given off by the exhalent arteries, which are so copiously dispersed on the surface of the lungs. A part is also emitted from the blood in the pulmonary vessels.

in quantity variable.

The estimation of its quantity is attended with some difficulty; according to Dr. Hales it amounts in a-day to 20 ounces: this is however but of little consequence, for it is liable to much alteration.

The blood undergoes important changes during respiration.

More important changes however than those just mentioned are produced by respiration, namely, the alterations produced in the blood; which fluid, returning from every part of the body by the veins, is poured into the heart; from whence, being propelled through the lungs, it is brought into contact with the air, undergoing certain changes which render it fit for the nourishment and support of the body. These changes, which are of a very complicated nature, have engaged the attention of several learned and ingenious philosophers. Dr.

It absorbs air; becomes florid red; emits carbonic acid; and water; and perhaps hydrogen.

Thomson has enumerated them as follows: 1. The blood absorbs air. 2. It acquires a florid red colour, and the chyle disappears. 3. It emits carbonic acid, and perhaps carbon. 4. It emits water, and perhaps hydrogen. Dr. Priestley, M. Lavoisier, and Lagrange, have each adopted a different the-

Varioustheories.

ory, by which they endeavour to explain and account for these changes produced by respiration: they are all however liable to considerable objections, and rest merely on the supposition that the oxygen is alone absorbed. Now Mr. Davy has shewn, that at least a portion of the azot disappears in the

\* Davy's Researches, page 433.



lungs: he has even rendered it very probable that the air is not decomposed, but that it is absorbed unaltered by the blood; that it is decomposed during circulation; and that the useless portion of azot is again given out. The following facts are in support of this opinion: "When the gaseous oxide of azot is respired, its quantity is diminished, carbonic acid gas is evolved as usual, and a quantity of azot makes its appearance. Now as this azot did not exist separately, it must have been produced by the decomposition of the gaseous oxide of azot; but its quantity being much less than the azot contained in the oxide of azot which had disappeared, it follows that a part of this last gas had been absorbed unaltered; and if a part, why not the whole? In that case the azotic gas must have been separated from the blood by the subsequent decomposition of the oxide of azot absorbed \*." Atmospheric air is composed of exactly the same ingredients as the oxide of azot, merely in different proportions, and in a state of less intimate chemical combination. It is moreover natural to ask, that if oxygen were alone absorbed by the blood, why should it not answer the same purposes as air? It is well known that this gas cannot be respired for a length of time without producing fatal consequences; but even when it is respired, the quantity (of oxygen) which disappears is much smaller than when a like quantity of atmospheric air is breathed for the same time. Mr. Davy has given the following experiment in proof of this fact: He breathed 182 cubic inches of oxygen gas for half a minute, 11.4 c. inches disappeared; whereas when the experiment was repeated under the same circumstances with atmospheric air, the quantity absorbed amounted to 15.6 cubic inches.

Mr. Davy apprehends that azot is absorbed with the oxygen, &c. Facts from the respiration of oxide of azot.

Oxygen alone is not proper for respiration;

Much less is absorbed than when a like quantity of atmospheric air is breathed.

It was first observed by Lower, that the colour of venous blood, which is dark reddish purple, was converted into the florid scarlet colour of arterial blood, in its passage through the lungs. The phenomena of respiration, however, still remained unexplained, until Dr. Priestley published his experiments on the changes produced in venous and arterial blood when put in contact with certain gaseous bodies. "† He observes, that having introduced pieces of the crassamentum of

Lower first observed the change of colour in venous blood by respiration.

Priestley shewed the cause; viz. the absorption of oxygen.

\* Thomson's System of Chemistry, Vol. IV. page 719.

† Priestley on Air, Vol. III. page 71.



coagulated sheep's blood into dephlogisticated air (oxygen gas); the blackest parts assumed a florid red colour, and that more readily than they would have done if common air only had been made use of: Whereas the brightest red blood became presently black in any kind of air unfit for respiration, as in fixed air, &c.; and after having become black in phlogisticated air (azot), it regained its red colour on being brought into contact with common air, the same blood becoming alternately black and scarlet, by being transferred from phlogisticated into dephlogisticated air, and *vice versa*."

These then may be regarded as the experiments which gave origin to all subsequent enquiries.

Description of  
the process of  
nutrition.

The food which is taken into the body is converted into chyle and excrement\*; the former of which is absorbed by a set of vessels termed lacteals, which convey their fluid into the thoracic duct. The term lymph has been applied to that fluid which lubricates the surfaces of all the circumscribed cavities of the body: This fluid is absorbed by a set of vessels termed lymphatics, which of course originate in every part of the body; they likewise terminate in the thoracic duct, which therefore is the great reservoir of the absorbent system; it receives the chyle and lymph, and conveys them to the blood; they are here decomposed, and converted into new substances necessary to the support of the body. Now the coagulable lymph, or fibrina, appears to be the most essential part of the blood, for it is employed to supply the waste of the muscles, &c. and Dr. Thomson has accounted for its formation in the following manner†: "It follows," says he, "from the experiments of Fourcroy, that fibrina contains more azot and less hydrogen and carbon than any of the ingredients of the blood, and consequently also than any of the ingredients of the chyle. In what manner the chyle, or a part of it, is converted into fibrina, it is impossible to say: We are not suffi-

Formation of  
fibrina.

\* The food, on being received into the stomach, is converted into a pulpy substance termed chyme. This alteration is effected by a peculiar fluid called *gastric juice*, which is secreted by the internal coats of the stomach. The chyme thus formed is propelled into the duodenum, where it meets with the bile, which converts it into a fluid much resembling milk, termed chyle, and into excrement.

† Thomson's Chemistry, 2d Edit. Vol. IV. page 725.

ciently

ciently acquainted with the subject to be able to explain the process. But we can see at least, that carbon and hydrogen must be abstracted from that part of the chyle which is to be converted into fibrina, and we know that these substances are actually thrown out in respiration. We may conclude then that one use of the air absorbed is to abstract a quantity of carbon and hydrogen from a part of the chyle by compound affinity, in such a manner that the remainder becomes fibrina : Therefore one end of respiration is to form fibrina.

Use of respiration in that connexion.

It appears then, from the above-mentioned facts, that the perfection of the blood is almost totally dependent on respiration; whenever therefore this function is suspended but for a very short time, death is the consequence.

Life cannot subsist without it.

It is well known that all the more perfect animals possess a temperature considerably higher than the surrounding atmosphere; the cause however of this increased temperature, remained unexplained for a considerable time. At length Dr. Black's theory of latent heat became known, when several attempts were made to explain the cause of the increase of temperature, or standard heat of the body, but none of them were satisfactory. Dr. Thomson has however given us the following ingenious theory : As the air is absorbed unaltered by the blood, it is evident that it will give out the greatest portion of its caloric during circulation; that portion therefore which is emitted at the instant that the air combines with the blood, is united to the carbonic acid, converting it into the state of gas, and the water into vapour. It appears moreover, that the heat of the blood is somewhat raised during circulation; for Mr. John Hunter found that the blood in the heart was a degree higher than in any other part of the body.

The elevated temperature of the more perfect animals is caused by the combination and condensation of air.

From the facts which have now been alluded to, it appears that the following changes are produced by respiration; The blood is propelled, by the contraction of the heart, into the pulmonary artery, which, by its numerous ramifications, conveys the blood into the small branches of the air-cells of the lungs, which are of so fine a texture as to admit the absorption of a portion of air. The blood having undergone this alteration, is returned into the heart by the pulmonary veins, from whence it is circulated over the whole body. During the circulation, the air which has been absorbed undergoes a gradual decomposition; carbonic acid and water are formed, which, together

Recapitulation,

together with a portion of azot, are returned by the veins, and thrown out as the blood passes through the lungs. A fresh portion of air is at the same time absorbed, and the above changes repeated.

These then are the effects of respiration, as far as we are at present acquainted with them; but this important branch of physiology still remains in considerable obscurity.

*Arlington Street,  
April 29, 1805.*

WILLIAM BRANDE.

#### IV.

*Instruction on the Processes discovered by M. BRALLE, of Amiens, for watering Hemp in Two Hours Time, in all Seasons, without injuring its Quality \*. Published by Order of the Minister of the Interior of France.*

Experiments on hemp, by order of the French government.

IN the month of Fructidor in the year XI. (September 1803.) the government called to Paris M. Bralle, of Amiens, the inventor of new processes for watering hemp. This discovery, which is interesting to agriculture, manufactures, commerce, and the marine, had engaged its attention; orders were given to make the experiments requisite to ascertain its value.

Every thing which could elucidate the principles and practice of M. Bralle's processes, which could prove and insure their success, was put in practice. Numerous and varied trials were made in the presence of M. M. Monge and Berthollet, senators, and Teissier, member of the Institute. M. Molard, administrator of the conservatory of arts and manufactures, directed these trials, and carefully pursued them for six months. The results were equal to the expectations that had been formed.

From the account rendered to his imperial Majesty, it was judged that the knowledge of a more expeditious method of watering hemp, than those employed at present, which is practicable at all seasons, and is in no respect injurious to health, by means of which a greater produce can be obtained from an equal quantity of the materials; and, which must extend

\* From *Bibliothèque Physico Economique*, Brumaire, An. XIII, and



and multiply the culture of an extremely valuable plant, could not be too extensively published. In conformity to this desire, we shall briefly describe M. Bralle's processes, relate the experiments which have been made, and offer some observations on the utility and advantages which are promised by this new discovery.

## § I.

*The Processes of M. Bralle.*

The means used by M. Bralle for watering hemp, are

1st. Water is heated in a vessel to the temperature of from 72° to 75° of Reaumur's thermometer; (200° fahr.) M. Bralle's process. The hemp is steeped in hot water with soap.

2nd. A quantity of green soap (*sûvon verd*) is added proportional to the quantity of the hemp to be steeped.

3rd. The hemp is then immersed so that it shall be covered by the fluid, after which the vessel is closed, and the fire put out.

4th. The hemp is left in this state of maceration for two hours, and then taken out.

The weight of soap required for a complete steeping, is to that of hemp-stalks as 1 to 48; and the weight of the hemp to that of the water as 48 to 650. Proportions of the articles.

Several steepings may be made one after the other. It is sufficient, before each new immersion, to add a quantity of soap water to replace what was absorbed by the preceding, and to raise the temperature of the bath to the above degree. The same water may be thus employed for fifteen successive days. The process may be repeated with other hemp in the same water.

When the bundles are taken out of the steeping vessel, they are covered with a layer of straw, that they may cool gradually, without losing their humidity.

Next day they are spread on a floor, pushing the bands towards the top of the stems, and a roller of stone or wood, loaded with a weight, is passed several times over them, to crush them, and dispose the tow to be easily separated from the reed, which is effected by beating. Whether the hemp be wet or dry, it peels completely in either state. Breaking the stalk, beating.

After having tied the handfulls of the tow peeled off while wet, at the top, they are spread on the grass, turned, and, after five or six days, carried to the warehouse.

The

The handfulls of steeped and crushed hemp which are intended to be beaten and stripped dry must also be exposed on the grass; this operation being absolutely necessary to whiten the tow, and facilitate the separation of the reed.

## § II.

### *Recapitulation of the Experiments.*

The experiments were varied to ascertain the requisite

Temperature,

time, and

proportion of soap.

Results or general observations on the process.

By means of a portable steeping vessel, different quantities of hemp were steeped, the temperature of the soapy liquor was varied at pleasure, and the state of the hemp was observed during the course of each operation, of which the duration was more or less prolonged, in order to ascertain;

1st. The temperature which the soapy liquor ought to have before the immersion of the hemp;

2nd. The time necessary for a complete steeping, at a determinate temperature;

3rd. The quantity of soap absolutely necessary for a given weight of hemp-stalks, weighed before the immersion, &c.

From a great number of experiments made in the months of January, February, and March last, it was found,

1st. That water containing the quantity of green soap directed by M. Bralle, for a given weight of hemp, effects the steeping completely;

2nd. That the steeping is so much the more speedy as the temperature of the fluid is nearer to ebullition, at the time of the immersion of the hemp;

3rd. That if the hemp be kept more than two hours in the steeping vessel, the time prescribed by M. Bralle for obtaining a complete watering, the tow separates equally well from the reed, but it acquires a deeper colour, and loses part of its strength;

4th. That if the hemp be immersed in a cold soapy liquor, and heat be then applied, the steeping is not accomplished so perfectly, whatever degree of temperature may be given to the liquor, and however long the immersion may be continued;

5th. That the bundles of hemp immersed and kept vertically in the vessel, are steeped more uniformly than if they were laid horizontally; and this position also facilitates the manipulation.

\* Probably the fluid between the fibres is not heated, because its conducting power is bad, and it is prevented from circulating. N.

## § III.

## § III.

*Observations on the Utility and Advantages of the New Discovery.*

Two methods only of steeping hemp are generally practised. Description of the methods of steeping hemp, as heretofore practised. Exposure on the grass.

The first consists in spreading the plant on the grass, and turning it two or three times a week, until the air, the light, the dews, or the rains, have disposed the tow to separate easily from the reed. The result is obtained in a longer or shorter time, according to the weather and the state of the air; and frequently, in certain countries, the operation is not finished in less than forty days.

The second consists in immersing the bundles of hemp in 2. Steeping: rivers, brooks, ditches, or pools, and keeping them there for eight, fifteen, twenty, or even thirty days, according to the degree of the heat of the water, or of the atmosphere.

The maceration effected by both these processes is frequently incomplete, and always unequal. By following the first, the cultivator is liable to have his crop dispersed by the winds, or injured by long rains: if he adopts the second, he runs the risk of losing a part of it by the overflowing of the rivers, or of its being covered with mud. The first method in particular, is liable to the serious inconvenience of depriving the national marine of part of the hemp produced by our territory: it is known that the tow produced from the hemp which has been exposed on the grass is not used by the government. These processes very defective.

The steeping of hemp according to M. Bralle's process, Superiority of the new process. requires only a copper cylindrical vessel, placed on a small furnace of bricks.

A steeping vessel of this kind, containing 240 litres of water, (52 ale gallons) is sufficient to steep 18 kilogrammes of hemp-stalks, (about 40lb.) and as the operation is completed in two hours, 100 kilogrammes (221lb.) may be easily steeped in a day.

This method appears to deserve the preference over the former ones, on many accounts.

1st. The steeping is practicable all the year, except during very hard frosts, when it is difficult to dry the hemp. It is practicable all the year. But when it is to be peeled wet, the cold is no longer an obstacle; it is then only necessary to take proper precautions for preventing the tow from freezing in its humid state.

2nd. The



saves time,

2nd. The time of steeping being only two hours, affords a saving of time of great value to the cultivator, particularly during the season of harvest.

and is not injurious to health.

3rd. The workman has no cause to fear any injury of his health: it is sufficient to keep up a current of air while the bundles are plunged into and taken out of the steeping vessel; the handfulls of stalks or tow, which are afterwards exposed on the grafs, do not emit any bad smell, or vitiate the purity of the air, whatever may be the quantity of hemp dried at once in the same place.

Every one knows, that when the bundles of hemp steeped in water in the old method, are taken out and washed, they emit an infectious odour which becomes insupportable during the heats, and to which serious disorders are ascribed. The valley of the department of the Somme, and many others in which hemp is steeped, afford too convincing proofs. The waters are rendered unfit for the use of cattle, and the fish contained in them are frequently destroyed.

Apparatus on a larger scale.

To accelerate the operation of steeping by the new process, in countries where there is an extensive culture of it, instead of the portable steeping vessel which was made use of in the experiments, the following apparatus may be adopted, consisting of a boiler and four wooden tubs, serving for steeping vessels.

After having heated the soapy matter to ebullition, it is suffered to flow through a cock, into two of these tubs filled with bundles of hemp, and closed by a cover: while the steeping is going on in the two first tubs, the necessary quantity of liquor is heated, to be conveyed into the other two, which are also filled with bundles of hemp, and closed with lids.

By means of this very simple apparatus, a considerable quantity of hemp may be steeped in a day without interruption.

Comparative expence of the two processes.

4th. The expence of steeping in water, compared with that required by the method of M. Bralle, is nearly the same, when the small steeping vessel is made use of; but if a cauldron, rather large, and the steeping tubs which have been mentioned are employed, the cost will be diminished more than a half.

In fact, the expence of the first includes the conveyance of the hemp to be steeped, the time employed in forming the bundles of hemp into a sort of rafts, that they may be sunk by

by loading them with stones, turf, clods of earth, and even mud; in fixing and securing these rafts by driving in stakes; a tedious work, and the more troublesome, because 10 kilogrammes of hemp-stalks cannot be immersed without a weight of 15 or 20 kilogrammes, and, after the steeping, all this mass must be removed, to take the bundles out of the water, and wash them.

The cost of the new process consists principally in the price of the solvent made use of, which amounts to about eight centimes for a kilogramme of tow. To this should be added the price of combustible necessary for heating the liquor, if this combustible was not afforded by the reeds of the bundles, whether they are peeled wet or dry.

At an equal expence, the new process is still preferable to the old, because, from what has been said, it renders the manipulation more expeditious and more easy.

5th. Eight kilogrammes of hemp-stalks steeped by the new comparative process, commonly produce two kilogrammes of pure tow, <sup>duce.</sup> by peeling when wet; whereas hemp steeped in water by the old process, and beaten, does not yield more from eight kilogrammes than one and a half.

The dry peeling of hemp steeped in the old way does not produce the same quantity as that which is peeled when wet; the breaking of the reed in many places occasions a greater loss of tow.

The hemp being washed, beaten, and combed in the old method, a kilogramme of long tow is obtained from four kilogrammes of the rough tow; the remainder is short stuff, hards and dust.

The same quantity of hemp, manipulated in the new way, yields two kilogrammes of long tow, one kilogramme of second tow, and about a kilogramme of short stuff and hards.

Thus from eight kilogrammes of hemp-stalks, two kilogrammes are obtained in rough tow by the new process, and from this quantity is obtained one kilogramme of the first tow, which does not exist in any known manipulation.

6th. The inhabitants of the banks of rivers and of the valleys, are almost the only persons who cultivate hemp; <sup>Extension of the culture of hemp.</sup> they owe this privilege to the vicinity of the waters, and the humidity of the soil. By the new process the culture of hemp will be extended to all places, and procure a new and very

very advantageous occupation to the inhabitants of the plains, the land of which is much more vegetative than that of the marshes.

It is an error to suppose that hemp cannot grow to a great height in the plains; it is a fact, that it rises to the height of two yards, in land which has been well ploughed and manured, when mild rains have promoted germination and growth.

It is equally a fact, that there is every where a sufficient quantity of spring or cistern water to steep the hemp by the new process: if droughts should supervene, which besides are only accidental, the steeping may be deferred.

It will, therefore, be possible to cultivate hemp in the plains, and in low lands, which are always rich and fertile, though frequently without springs of water, and to augment not only the mass of our products, but also our riches of this description, since one acre of good hemp yields as much profit as two acres of wheat.

Summary of the  
advantages  
arising from this  
process.

Such are the effects which may be expected from M. Bralle's new method of steeping hemp. It is, as was observed at the commencement of this instruction, more expeditious than those hitherto employed; it perfectly completes the steeping; it may be used at all seasons; it does not affect the purity of the air; from an equal quantity of materials, it procures a more abundant produce; and lastly, it is well calculated to extend the cultivation of the plant itself. The enlightened lovers of agriculture, and well-informed proprietors, who live upon and cultivate their own estates, without being slaves to the customary practices, will adopt it, and secure its advantages, by repeating the experiments which have ascertained its merit, and also by making trials on a more extensive scale than those which took place in the conservatory of arts and manufactures. Their example will be followed, the process of M. Bralle will be extended, and we shall see portable steeping vessels, similar to those used by M. Molard, multiplied; a cheap apparatus which requires very little repairs, and by means of which the hemp grown through the extent of one or of several communes may be steeped even in the field on which it grew.



## V.

*Description of a Portable Steam Engine. By Mr. MATTHEW MURRAY.*

To Mr. NICHOLSON.

S I R,

I TAKE the liberty of handing you the description of a portable steam engine of my construction, which you will have the goodness to insert in your Philosophical Journal. I will just observe it is reduced to the fewest parts that practical utility will admit, which must necessarily render it of great advantage; as the simplicity of its parts make it nearly impossible to be out of order with a very moderate degree of management. The following description and reference to the plate will explain the nature of this engine.

I am, Sir,

Your much obliged humble servant,  
MATTHEW MURRAY.

*Leeds, May 7th, 1805.*

*Description of a Portable Steam Engine.—Plate VII.*

AA Represents the ground or floor on which the engine stands. *Description of a portable steam engine.*

B Section of a recess made in the ground for the beam O to work in.

C. Iron cistern resting upon the ground or floor covering the recess for the beam.

D An opening in the floor to admit a boy to oil the centers of the beam.

E A double steam cylinder, having an upright pipe in the intermediate space, which effects a communication between the top and bottom and the valve box G.

F A steam pipe that communicates with the boiler through which all the steam passes and surrounds the inner cylinder in its way to the valve box, prior to its application against the piston.

G The valve box fixed upon a projection from the cylinder bottom, having an opening or connection with the interval between the two cylinders. In this opening is a regulating valve

Description of  
a portable steam  
engine.

valve for adjusting the quantity of steam (that acts against the piston) in its passage through the valve box. There are also three other openings in the bottom of this valve box, one of which connects with the top of the cylinder by the pipe in the intermediate space, the second with the bottom, and the third with the eduction pipe that leads to the condenser. Two of these openings are alternately connected together by a slide valve,\* while the third is left open for the admission of steam to the piston, this valve changes its position at the end of each stroke of the piston, and performs all the purposes of the most complicated machine.

H The air-pump connected with a condenser at the bottom of the eduction pipe.

I. The fly wheel fixed upon an axis which receives its motion from a crank connected with the beam by the rod K.

LL Two rods for connecting the motion of the piston to the beam, these rods move perpendicularly by a motion which could not be conveniently shewn in this view without rendering it confused.

M A spherical triangle turn'd by the crank for moving the slide valve by the horizontal rod N that connects them together. This motion has the advantage of preventing the engine from ever turning the contrary way round from that which it is wanted to go, and prevents the noise that is usually heard in engines.

O The beam attached to the bottom of the cistern C by means of the hanging carriages P.

Q A rest or fixture in a wall for the end of the fly wheel shaft; this will vary according to the situation where the engine is to be fixed, or it may be supported by a metal standard.

R. Index to the injection cock that admits water to the condenser. *Note*, The cistern is to be kept nearly full of water during the time the engine is at work.

The cylinder G and valve box E must be surrounded on all sides by a case (not shewn in this view) the space between filled with charcoal to prevent the transmission of heat, which if effectually done will work with the least possible quantity of coals, as it combines the advantages of every other engine hitherto known. By detaching the air-pump and condenser

\* For these valves I took out a patent in 1802.

(which

(which may be done in half an hour) and where water cannot be had for condensation, this engine may be worked by the pressure of strong steam alone, as the internal cylinder is kept as hot as the steam in the boiler. \* This dangerous plan never ought to be resorted to but in cases of necessity, as it is no saving of coals, and as there can be no certain rule when to discontinue the use of the boiler, the weakness of which is not prevented by putting the fire in a tube in the inside of it. This engine requires no framing nor mill-wright work in the fixing, but merely bolting down to the floor it stands upon. It takes up very little room, and all its parts are within reach, without the necessity of upper floors or stages, which would be the case if the beam was above; but by being fixed below and alone, it has no tendency to move from its situation.

Description of  
a portable steam  
engine.

## VI.

*Letter from Mr. J. C. HORNBLOWER, Engineer, on the Measure of Force by Horse Powers.*

To Mr. NICHOLSON.

DEAR SIR,

I AM induced to trouble you on account of the present unsettled state of things respecting what is usually called *the power of a horse*. I do not know why a matter of this sort should remain so disregarded, especially as it has so long become one of our data, comprehending the unities of weight, space and time, by which we are to be understood in our communications on the subject, and by which we are to ascertain the pre-

Uncertainty of  
what is meant  
by the power of  
a horse.

\* Many engines are at present worked in London and elsewhere by the mere force of steam, without condensation, under Trevithick's patent. The force is from 45 to 60 lb. on the round inch; a pressure equal to about 25 fathoms of water at the most. Various assertions and reports concerning the safety, the economy, and the other effects of these engines have passed under my notice; but the interested situation of some of the narrators on both sides, and the short time of trial, have induced me to wait for more facts before I should give any account of the engine in this Journal. I hope to do this a few months hence.—W. N.

cise



cise value or effect of any mill or engine in and about London. Indeed I do not know why it was adopted for the purposes intended, it being so indefinite.

This unity probably arose from steam engines being substituted for horses.

I can easily conceive it probable that somebody who has employed horses for some time in mill-work, may have applied to an engineer, and said "I have a mind to have my work done by a steam engine instead of horses, for I am to a point that I shall save money by it, and please to give me an estimate of the cost of an engine that will do the work of my horses;" and then the engineer sets about getting information as to what may be deemed the effect a horse can produce, and calls it the *horse power*; and perhaps having Defagulier's Experimental Philosophy at hand, applies to him, and there he finds that a horse will raise a hoghead of water 50 feet high in a minute; then what is the weight of a hoghead of water, and he finds from some particulars related by him, 2nd vol. page 505, that a hoghead of water is equal to 550 lb. but of what measure is uncertain, for the ale hoghead, 51 gallons, is 540 lb. and the wine hoghead, 63 gallons, is 504 lb. reckoning the cubic foot at 1000 ounces avoirdupois.

Defaguliers considers it as 550 lb. raised 50 feet per minute.

Another estimate; nearly double that of Defaguliers.

Some engineers who have very unceremoniously taken the lead in this affair, have adopted I do not know what for a *datum*, but the result is this: An engine by calculating 10 lb. on the square inch, making the whole pressure = 1000, moving through 200 feet per minute, is called a *four-horse engine*.—Let us see then what will be the effect of one horse according to this fact.

$$1000 \text{ lb.} \times 200 \text{ ft.} = 200000 = \text{the whole effect, then } \frac{200000}{4}$$

= the effect of one horse. Now compare this with the estimate of Dr. Defaguliers, which is a hoghead of water at 550 lb. 50 feet high in a minute;  $550 \text{ lb.} \times 50 \text{ ft.} = 27500$  and  $50000 - 27500 = 22500 =$  the difference between one of Mr. Watt's horses and one of the doctor's, on the former of which I make no comment.

Smeaton's estimate one-fifth less than Defaguliers.

Mr. Smeaton, whom I hold as having superior claim to pre- cedency on subjects of this nature, has utterly disapproved of Defagulier's experiment by the most powerful conviction of its fallibility, formed by conclusions drawn from sterling experience in the accomplishment of works on a large scale; and he states the greatest effect to be 40 feet high in a minute; but

as this is still in the commonly received opinion as to the weight of the hoghead, I would rather turn to those who have made their experiments on a weight of solid matter, expressed in terms which cannot be mistaken.

I remember to have had some conversation on this subject many years ago, with the late Samuel More, at that time secretary to the Society for the Encouragement of Arts, &c. when he shewed an instrument constructed on purpose to determine the resistance against horses at plough. I do not recollect that I made any minutes on the result of our conversation, so can only say that his relation of the fact surprised me, until I came to compare it with the effect of horses actually applied to overcome a load drawn up a shaft in a mine; but I had not the same means of determining the re-action that he had; however, the result of his experiment may be seen in the Transactions of that Society; some observations on it may be seen in the 3d vol. quarto, of the Philosophical Journal, page 136, only there seems to be a mistake in the deduction in the note: it should be  $264 : 10 :: 1375 : 52 +$

Mention of experiments on the reaction against horses, by the late Sam. More, Esq. with an instrument.

*It was a spring with graduation.*

I much wish to have an experiment like Mr. More's made by a sledge drawn forwards and backwards on a level road, with Mr. More's instrument placed between it and the horses; such an experiment would be very practicable, and the small deviation from the true level of a road would be compensated by alternately going first one way and then the other.

Proposed experiment.

It is true that we are become pretty well acquainted with what may be done by horses in grinding malt, pumping liquor and worts in breweries; but there are so many fortuitous circumstances to be regarded, even here, that nothing decided can result from the closest investigation. For instance, some brewers chuse to have their malt ground much lower than others; the pump-work is executed in some breweries under very different advantages, and from local circumstances may be retarded by the inertia necessary to communicate motion from the wheel to the work, adding the different condition of valves, buckets, &c. All these considerations demand some invariable resistance to be overcome by the exertion of the horse, and I know of nothing so appropriate as that I have just mentioned.

Uncertainty of the measure of a horse power from grinding or pumping, &c.

The power of a horse (by which I mean the mechanic power) is not easily ascertained. It has scarce any analogy

Difficulty of this subject, not only from the work, but the



class of horses  
employed.

with a weight descending through a given space or a quantity of water falling a given height, and therefore is better expressed by the terms effect, resistance, re-action, &c. and even then, to be any thing like precise, we ought to discriminate whether brewers' horses, or higlers' horses, waggon horses or coach horses, heavy horses or light, and if you will go into the country among the coal-mines, you will have another class of these animals, which I know not what to call unless it be *poor horses*, full worked and half starved; in short, I mean that neither one or the other ought to be taken into the account as the *measure of a mechanic power*.

Mr. More's  
estimate of 80lb.  
3 miles an hour.  
This is nearly  
three fourths of  
Defagulier's  
rate.

Nevertheless it seems desirable to have some popular expression for the application of whatever may be substituted in the place of horses, whether steam, water or wind; nor can there be any objection to saying, "equal to the work of so many horses," provided we can attain to a clear, unequivocal and somewhat exact value, attributable to that power, and if I may give my own opinion, I think Mr. More has stated the utmost effect to be 80lb. three miles per hour, in such horses as are proper for giving motion to mill-work, and at such spells as will not exhaust the breath or strength of the animal.

Remarks.

I am surprised to find this mode of calculation has obtained so far as to determine the power engines employed purely as pumping engines, as lately at the Tunnel, the New Docks, &c. but I am glad it reaches no further than the bills of mortality, and I hear that the Dutch method of hoisting goods to warehouses has lately been adopted at some of our new docks. *O tempora, O mores!* While other countries are availing themselves of the application of the steam engine in place of animal labour, we are taking up the expedients of those who have scarce heard there is any such thing as a steam engine, or who cannot appreciate its value on that degree of evidence we have in our own country.

Whether the  
horse power or  
unity be true or  
not, it ought  
surely to be free  
from ambiguity.

It may be objected to by some to alter the present data, however erroneous, as we shall be obliged to require 20 horse engines instead of 10 (for it appears the estimate is nearly, if not quite *cent. per cent.* more than it should be); but even that can make no difference in any respect than as making a rent in an egregious error; the cost of an engine cannot be altered by it, nor the consumption of fuel, but a material convenience would be the result of such a regulation, considering the ad-

vantage



vantage of a coincidence in this point throughout the kingdom, and as partaking of the nature of a unity of weights and measures, it ought to be paramount to every subordinate consideration.\*

Your much obliged obedient servant,

J. C. HORNBLOWER.

VII.

*Letter from Mr. A. F. THOELDEN, communicating three manuscript Tables from Mr. BODE, of Berlin, of the geocentric Places of the new Planets Ceres, Pallas, and Juno, for twelve Months to come.*

To Mr. NICHOLSON.

SIR,

THE three planets, or afteroids (according to Dr. Herschel), lately discovered, being so very small, are not easily found, unless the observer is acquainted with the place where he is to look for them. This uncertainty induced some astronomical gentlemen to desire me to inquire, if there were not any ephemeris of their motions published abroad. I complied with their request, and Mr. Bode has very obligingly communicated to me the following written account of their respective situations, calculated for the Observatory at Berlin, (that of Juno, according to a table of his own calculation). If you think this communication may be interesting to the astronomical readers of your Philosophical Journal, I beg you will make use of it.

I am, Sir,

Your most obedient humble servant,

10, St. Alban's Street,

A. F. THOELDEN.

May 13, 1805.

P.S. A new edition of Mr. Bode's small Celestial Atlas has just been published; with a Catalogue of 5500 Stars, after Piazzi's observations. This last work can be had separate.

Any gentleman who may be desirous of one or both these works, will be supplied in a reasonable time after sending an order to me.

\* For a very clear and rational report of a steam-engine in horse-powers, see our Journal, IX. p. 215.—W. N.

TABLE I.

GEOCENTRIC MOTION OF CERES.

1805.	A. R.	Decl.	1806.	A. R.	Decl.
Oct. 2.	104° 34'	22° 51' N.	Jan. 9.	103° 26'	29° 46' N.
11.	106 53	23 3	18.	101 11	30 25
20.	108 49	23 18	27.	99 18	30 53
29.	110 21	23 39	Feb. 5.	97 56	31 11
Nov. 7.	111 24	24 5	14.	97 13	31 22
16.	111 54	24 40	23.	97 10	31 26
25.	111 49	25 22	Mar. 4.	97 46	31 25
Dec. 4.	111 7	26 12	13.	98 58	31 20
13.	109 48	27 7	22.	100 43	31 11
22.	107 59	28 3	31.	102 57	30 58
31.	105 46	28 58	Apr. 9.	105 32	30 40
			18.	108 28	30 18
			27.	111 40	29 51
			May 6.	115 6	29 19
			15.	118 42	28 40
			24.	122 27	27 56

## TABLE II.

## GEOCENTRIC MOTION OF PALLAS.

1805.	A. R.	Decl.	1806.	A. R.	Decl.
Aug. 3.	59° 29'	3° 4' S.	Jan. 3.	68° 34'	31° 16' S.
12.	62 47	4 22	12.	67 51	29 27
21.	65 54	5 56	21.	67 48	27 12
30.	68 52	7 47	30.	68 24	24 39
Sep. 8.	71 34	9 53	Feb. 8.	69 37	21 54
17.	73 58	12 15	17.	71 26	19 3
26.	75 59	14 50	26.	73 45	16 10
Oct. 5.	77 35	17 37	Mar. 7.	76 33	13 20
14.	78 40	20 30	16.	79 44	10 35
23.	79 12	23 23	25.	83 16	7 59
Nov. 1.	79 8	26 8	Apr. 3.	87 6	5 34
10.	78 26	28 38	12.	91 11	3 21
19.	77 11	30 43	21.	95 28	1 22
28.	75 29	32 13	30.	99 57	0 19 N.
Dec. 7.	73 33	33 3			
16.	71 35	33 9			
25.	69 51	32 32			



TABLE III.

GEOCENTRIC MOTION OF JUNO.

1805.	Longitude.	Latitude.	1806.	Longitude.	Latitude.
Oct. 1.	4° 28' 22"	6° 21' S.	Jan. 1.	5° 26' 39"	4° 18' S.
11.	5 12 28	6 12	11.	5 27 25	3 56
21.	5 6 18	6 1	21.	5 27 29	3 31
Nov. 1.	5 10 23	5 47	Feb. 1.	5 26 45	3 0
11.	5 13 54	5 33	11.	5 25 19	2 22
21.	5 16 44	5 21	21.	5 23 20	1 44
Dec. 1.	5 19 23	5 8	Mar. 1.	5 21 33	1 15
11.	5 21 34	4 54	11.	5 19 0	0 33
21.	5 23 27	4 38	21.	5 16 41	0 10 N.
31.	5 25 20	4 22	Apr. 1.	5 14 27	0 50
1.	5 27 13	4 6	11.	5 12 56	1 26
11.	5 29 6	3 50	21.	5 12 35	1 57
21.	5 30 59	3 34	May 1.	5 11 46	2 19
31.	5 32 52	3 18			
1.	5 34 45	3 2			
11.	5 36 38	2 46			
21.	5 38 31	2 30			
31.	5 40 24	2 14			

## VIII.

*Letter from Professor PINI, Inspector of Mines to the Italian Republic, to J. C. DELAMETHERIE, on Corindon found in Italy.\**

THE interest you take in publishing discoveries in natural history in your excellent Journal, induces me to communicate to you a mineralogical rarity lately found on a mountain of the Italian republic: it is a very fine corindon, or adamantine spar, of a deep ruby colour. I saw it for the first time among the minerals which the learned Brochi, professor of natural history at Brescia, had made a short time before in the department of Serio. At the first view he considered it as a felspar, of which it has all the appearance; and, in fact, the corindon being a substance which hitherto has only been furnished by countries far distant from us, it would have been imprudent to have judged otherwise at first: but the colour of stone, exactly resembling that of a red corindon which I brought from Paris, given to me as coming from Madras, led me to suppose that it did not differ from it.

Beautiful red  
adamantine spar  
found in Italy.

But as Professor Brochi purposed meeting me in a short time at Milan, to which place I was going, we postponed the verification of this suspicion. When he saw the red corindon from Madras, in my possession, he no longer doubted the identity of its species with that of our sample. I afterwards discovered the same identity in the trials to which I submitted it: the following are the results:

1st. The corindon of Italy scratches the hardest rock-crystal. Examination.  
2d. It does not melt before the blow-pipe, either alone or with the addition of borax. 1. Hardness.  
3. Its texture is in laminæ, which follow different directions. 2. Infusibility.  
4th. Its fissure is triple, and when 3. Laminar texture.  
it is cut in the three directions, it offers a rhomboid, the acute 4. Fissure and angle.  
angle of which is  $64\frac{1}{2}^{\circ}$ . 5th. Its cross fracture shews the 5. Reflection of light.  
splendor of the diamond, and reflects the light, the flashes of 6. Sp. gravity.  
which are almost the colour of silver, 6th. Its specific gravity is 3.87, which is the mean of that of the true corindon.

Hitherto it has been met with in a mountain of micaceous schistus, in pieces of several inches in length, which are amor- Is found on a mountain of micaceous schistus.

\* From Journal de Physique, Vendemiaire, An XIII.

phous and opaque, but semi-transparent on the thin edges. Professor Brochi and I purpose making new researches there, which may lead to some more interesting discovery.

Countries where  
corindon is  
found.

In the mean time it will no longer be doubted, that the corindon is a product of Europe. M. de Bournon, with whose memoir on corindon, inserted in the *Journal des Mines*, Vol. XIV. you are well acquainted, has noticed the different countries which furnish this substance; they are the island of Ceylon, the peninsula of India, and in particular Madras, the Carnatic, and China. He concludes his interesting details by enquiring whether this substance exists in other countries, except those acknowledged to be the chief, if not the exclusive situations of this species. This question arises from several stones found in Europe having been given as corindons. In fact, those collected in Germany were found to be sometimes feldspars, and sometimes the *schorlartiger-beryll* of Werner, your leucolite. That mentioned in the *Museum Britannicum* as coming from Tyrie, on the eastern coast of Scotland, was far from having the hardness belonging to this species; that from Chestnut-hill near Philadelphia, announced by Mr. Smith, was discovered by Mr. Richard Phillips to be a fragment of badly crystallized quartz. It only remained to decide on the feldspar found by Bournon in France, in the province of Forez, the description of which he sent to you in a letter inserted in the *Journal de Physique* for June 1789, and which he still considers as a true corindon.

The specimens  
of European  
stones called co-  
rindon do not  
belong to that  
species.

This substance appears to be the same as that you have called andalousite, and which some dealers in natural history have circulated in commerce by the name of adamantine spar, from the kingdom of Castile: it has been placed by Abbé Haüy in the appendix, which contains those substances, the nature of which did not appear to be sufficiently known to permit him to assign them a place in his method. He calls it apyrous feldspar. Thus we may be satisfied that hitherto there is no certainty of corindon having been found in Europe. I flatter myself that now there will be no doubt on the corindon of Italy which I have the honour to announce; it is not even deficient in the specific gravity belonging to this substance; a defect which induced Professor Haüy not to acknowledge the feldspar of Forez to be a corindon.



If rubies and sapphires be classed with corindon, as many mineralogists seem disposed to do, Europe will probably have mines of these precious stones as it already has mines of emeralds, such as those of Limoges, discovered by the learned Lelievre, counsellor of the mines. But these stones will be rubies, sapphires, and emeralds of mineralogists, but not of the jewellers, until they shall be found very transparent, which I hope will be the fruit of new researches, followed with perseverance.

HERMENEGILDE PINI.

### IX.

*On disclosing the Process of Manufactories. In a Letter from*  
Mr. JOHN CLENNELL.

To Mr. NICHOLSON.

MY DEAR SIR,

Newcastle, Feb. 17, 1805.

PERMIT me to intreat the attention of some of your numerous correspondents towards a question which must certainly be interesting to every manufacturer, but of which no regular discussion has yet been offered—Is it proper or improper to lay before the public in respectable periodical works, a full and impartial statement of the various processes of our manufactories? I shall state such reasons as have offered themselves to me why they should be displayed, but I am principally anxious to receive further information on a subject that appears to me peculiarly interesting.

Interesting question respecting the disclosure of manufacturing processes.

The first argument I shall adduce is that of Mr. Boyle, as quoted by Dr. Johnson in the 201st number of the Rambler.

“The excellency of manufactures, and the facility of labour, would be much promoted, if the various expedients and contrivances which lie concealed in private hands, were, by reciprocal communications, made generally known; for there are few operations that are not performed by one or another with some peculiar advantages, which, though singly of little importance, would, by *conjunction and concurrence*, open new inlets to knowledge, and give new powers to diligence.”

Argument of Boyle,

The

confirmed by experience.

The second is the very considerable improvements that have taken place in those few manufactories which have yet been under the influence of chemical enquiry; thus realizing, but on a very extensive scale, the suggestions of Mr. Boyle: so far therefore as we are to be guided on the one hand by experience, and on the other by the influence of scientific enquiry on liberal display, will the argument be in our favour.

Accidental discoveries improved by disclosure.

In the third place I would observe, that as many very valuable discoveries are owing to accident, those with whom they happen are frequently perhaps incapable of improving them to the extent they would admit of in the hands of men of science, and thus, by a spirit of monopoly, preclude even themselves from the advantageous cultivation of such discoveries, merely lest others might enjoy it also. If, again, we consider the rapid progress that has been made of late years in every department of useful and practical knowledge, we must attribute it entirely to those liberal communications that have been made by men whose attention has been immediately directed to the promotion and improvement of every thing valuable to the public. Again, the profits of every business depend on the regularity and knowledge with which it is conducted; but how is the last to be enjoyed without resources to apply to, and how much more easily would it be obtained if science could regulate and simplify the combinations of the manufacturer? If to accomplish by every thing employed (and even in many cases the refuse) in each process its utmost possible use, is a favourite principle with manufacturers; to take the most accurate and best adapted means to effect it, ought certainly to be as powerful with them. Is it not also obvious, that, to discard all mystery and quackery, and fairly to disclose each process, is to invite the attention of men of science and research to extend any advantages gained by chance or otherwise, and to discover greater utility in the various substances employed. The origin, progress, present state, and hints for the improvement of the "arts of life," would certainly be worthy the contemplation of our first chemists, and are subjects that have appeared of such importance to a neighbouring nation, that many of their most eminent men have been employed in them; and some volumes of the *Encyclopédie Methodique* are dedicated to such information, with plates too, in several instances displaying even the most minute work-tools employed in each.

Science would be thus introduced into workshops, &c.

The

The history and detail of manufactories conducted in each place. Some objections ought, I presume, to form a principal object with the writers of local histories; yet very few of those gentlemen are enabled to obtain such accounts as they can depend on, from the selfish and monopolizing jealousy of manufacturers in general. To these various advantages an objection may be offered, "That display is placing objects of taxation in the hands of ministers: be it so; display will make it easier to collect the tax, will make it more certain, and it may be, *less oppressive*: if to these be added the above advantages, it may fairly be presumed, that discovery and consequent improvement is the most advantageous track to be pursued; but, my dear Sir, I beg your pardon, on this subject I did not mean to offer my own opinion so much as to solicit information from that of others.

I am truly your's,

JOHN CLENNELL.

How far literary pursuits are compatible with the duties of the commercial man, or the manufacturer, seems a question so completely decided in the affirmative, in the first volume of the Manchester Memoirs, by Mr. Henry; in the second volume of the same work, by Dr. Barnes; and in the hundredth number of the *Lounger*,—that the above paper assumes the principle as being fully established.

J. C.

## X.

*Question respecting the Purification of Copper. By J. P. With a concise Reply. W. N.*

AS copper in its purest state (especially out of London) for manufacturing different articles, cannot be obtained without a very tedious process; as it forms the principal ingredient in mirrors for reflecting telescopes, and likewise is much used as an alloy for gold; if it is impure, it never fails to render the gold so alloyed brittle, and not to be restored to its ductility until the impure alloy be wasted by subsequent meltings, to the loss and disappointment of the workman. Required, therefore,

Question respecting the purification of copper.



therefore, a method of purifying the copper, particularly for the latter purpose?

Your's respectfully,

Morley Street,  
Newcastle-upon-Tyne.

J. P. Jun.

Reply.

As the processes for refining copper in the large way are grounded upon its property of resisting oxidation more than the other metals which are usually combined with it, it may be adviseable to adopt the process of Pelletier, with a due attention to the manipulation and the proportion of manganese to be made use of. The very interesting letter of Mr. Thomson in the present Number, will indicate the principles of operation. Mr. Hatchett's excellent papers in the Philosophical Transactions, of which a correct abridgement is given in our V. and VI. Volumes, shew the mischievous consequences of impurity in the copper for alloying the precious metals; and it is but too well known, that it is difficult to be procured, or even to be made pure, upon a scale of extensive magnitude.

W. N.

## XI.

*Description of a Method of connecting Iron Bars, and coating them with Lead, so as to form solid Pillars for Light-houses on Rocks covered at High-water, and to defend them from Corrosion. By Capt. JOSEPH BRODIE, of the Royal Navy.\**

Description of the means of coating iron bars: by reference to the drawing.

FIG. 1, Plate V. A shews four rods of cast iron, composed of a number of pieces two feet long, rivetted together, in a manner explained by the plate, so as to produce the effect of one bar of the thickness of the whole. B. A tube of cast iron, formed from a number of separate pieces, each about ten inches long, and which, when placed round the iron rods above-mentioned, and then screwed together, form a mould, into which the melted lead is to be poured, to coat the iron

\* Communicated to the Society of Arts (Memoirs, MDCCCIV. 258.) who voted him the gold medal.

rods.

rods. C. A portion of the rods covered with the melted lead, so as to form a cylindrical pillar apparently of lead, the iron being perfectly coated therewith.

*Fig. 2.* D shews the manner in which the hollow cylinder is formed to any length required, by the junction of a number of semi-cylinders rivetted together and fitting each other. E, the side flanges screwed close together. F, the end flanges also screwed together, as prepared for the melted lead.

After a certain portion of the iron rods are coated with lead, the lower parts of the tube are taken off and placed higher up; by which repeated changes, a few tubes will answer the purpose to coat any length of the iron rods.

## XII.

*Reply to Mr. Accum's last Letter on the Production of Nitrous Acid. By W. F. S.\**

To Mr. NICHOLSON.

SIR,

I THINK Mr. Accum by no means throws off the charge of censure with which he is accused, upon so material a part of the modern theory. He ought to have been more explicit, and in so nice and so disputed an experiment, to have given us a minute detail.

Remarks on  
Mr. Accum's  
letter.

I have performed the experiment which he speaks of, but I could detect no nitrous acid after the process. The air gradually diminishes by the electric spark, but this diminution is owing to the oxygen gas producing a calcination of the metals employed, for the purest oxygen gas answers better than when mixed with nitrogen. Therefore I hope, if Mr. Accum possesses a more accurate experiment, he will give it through your Journal: certainly the present state of modern chemistry requires a very minute investigation. I hope, Mr. Nicholson, you will not refuse inserting this in your Journal.

Unsuccessful at-  
tempts to repeat  
the experiment  
of forming ni-  
trous acid.

London, May 2.

\* See our Vol. X. p. 109 and 214.

## XIII.

*Experiments on the Electricity of Metallic Filings sifted through Metal; with Remarks in answer to a Letter of Mr. Cuthbertson. By Mr. WM. WILSON. (From the Author.)*

To Mr. NICHOLSON.

SIR.

An error pointed out by Mr. Cuthbertson admitted.

IN the last number of your Philosophical Journal, I find a letter from Mr. Cuthbertson, containing some remarks on my letter on the electricity of metals, in which he notices an error in the table of results of experiments, that had escaped my notice till I read his letter. In that table I have by mistake put P against copper filings sifted through zinc instead of N.

Copper filings when sifted through zinc are electrified strongly with negative electricity.

Mr. C.'s objection to the production of electricity by mere separation of metals, considered.

The metallic filings touch the sieve, are separated, and touch the receiving metal.

As the el. varies with the sieve it does not depend on the latter contact,

—and in the first contact there is no insulation,

—consequently it depends on the separation.

Mr. C. in his other remarks does not seem to admit that the separating the metals from contact is the cause of the electrical fluid being excited and not touching, because both touching and separating are employed. In the way the experiments were made touching takes place two ways, viz. the filings come in contact with the metal plate they are sifted into, and they are in contact with the sieve before they are sifted through it; but I cannot conceive how either of these contacts could cause the excitation of the electricity in the experiments. If the contact of the filings with the plate they were sifted into was the cause, we should have had the same effect with the same filings, whatever metal the sieve was made of they were sifted through; because as they were always sifted into the same plate, the same metal filings always came in contact with the same metal plate, and consequently we should have had the same effects in all cases with the same filings; whereas out of the ten sorts of filings that were used there was only one (steel) that produced the same effect with the different sieves; and if the contact of the filings with the sieve excited any electric fluid, it would be dissipated as fast as excited, because neither the sieve nor the filings were insulated; consequently it could have no effect on the results of the experiments.—Therefore since neither of these contacts excited the fluid, it must have been excited by the separation.

I think



I think the following experiments will put this beyond a doubt: 1. I fastened a piece of card into a stick of glass, and then rubbed it over with strong gum water and covered it with filings of zinc; so that when it was dry, it had a surface of filings of zinc. From a heap of the same filings I took up as much as I could on this little shovel without touching it with any thing else, and let them fall very slowly upon a piece of bright sheet copper fastened in an inclined position upon the cap of an electrometer, and formed into a receptacle at its lower part to contain the filings. In this operation almost every particle of filings necessarily came in contact with the face of the copper. After letting fall upon the copper about an ounce and a half of filings there was not the least sensible effect on the electrometer. 2. I then took a piece of the same sheet copper, which was pierced full of small holes, and sifted through it the same filings I had used in the above experiment, upon the same copper on the electrometer, and the gold leaves diverged with positive electricity and discharged themselves against the slips of tin foil on the inside of the glass ten times before the whole of the ounce and half of filings were sifted into it.

An insulated shovel of card, faced with zinc filings was used to pour zinc filings on a bright plate of copper.

The copper plate was not electrified.

A copper sieve was then used instead of the shovel.

The copper plate was strongly positive.

Now, since the separating the two metals from contact in the second of the above experiments is the only difference between them, and as the electric fluid was excited only in the second, I think we may safely conclude that that separation was the cause of the excitation, and not touching.

Separation was alone the cause of this electricity.

I am very much inclined to believe that the excitation that takes place in friction is caused by the same circumstance, and that the friction does nothing more towards the excitation than bring the different parts of the substance that are rubbed together into contact, and separate them from it.

Electric excitation is probably a fact of this kind.

I am your obedient humble servant,

WILLIAM WILSON.

## XIV.

Reply to Mr. Boswell. By AN OLD CORRESPONDENT.

To Mr. NICHOLSON.

SIR,

Explanatory  
remarks on Mr.  
Boswell's letter.

WHEN I began to read your correspondent, Mr. Boswell's answer to my observations on his geometrical propositions, and found myself accused of having "thrown some very undeserved reflections" on his communication, I could not help feeling a degree of apprehension lest I should inadvertently have made some mistake or other, for which I must have been obliged to apologize to him and to the public; but on perusing the letter through, I was not a little surprised to have found but *one* reflection pointed out, and that applying not to the *matter* but *manner* of my observations; it seems I have accused him of being *too confident* in one of his assertions; a literary crime, of which he exculpates himself by proving from a quotation, certainly very much to the purpose if we make no distinction between *doubt* and *diffidence*, that he is, on the contrary, a very *diffident* writer; which quotation, to be sure, contrasted with an expression that fell from my pen, might be conclusive, if the two passages were applicable to the *same thing*; but unfortunately it turns out, on closer examination, that the accusation applies *exclusively* to Mr. B's *second* proposition, and his exculpatory quotation *exclusively* to his *first*.

Had I committed myself so far as to say that he announced his first proposition with confidence, I must have stood clearly convicted of having done him injustice; but as it was his second proposition, or, in other words, his *other fact* in geometry, to which my objectionable observations *solely* applied, he ought to have quoted what he has said about *it*, and about *it only*, as evidence against me; he has, however, directed his arrow at a wrong mark, on which account I claim the reader's indulgence to repeat the passage alluded to, which is the *only* passage in Mr. B's paper that relates to his second proposition: "The discovery of a fact in geometry often leads to *another*; one of this kind *I have here to add, which is*, that a right line (BE) drawn from the extremity B of the line IB, at right angles through the opposite diameter (IF) to the circumference,

ence, *will be equal* to a fourth of the circumference." In this annunciation, which is an appendage to the main subject or first proposition, and evidently not included in the prefatory apology, I see nothing like a suspicion of inaccuracy expressed, or even hinted at, and consequently can trace no mark of *diffidence*, now that I come to examine it again; on the contrary, I repeat, that a fact is confidently asserted to exist, which has been proved *not to be a fact*, both by myself and by Mr. Gough, to whose testimony probably the reader will pay some deference, particularly as this gentleman has ingeniously shown that *another line* in the circle possesses the identical property erroneously attributed to the line BE, and that previously to Mr. B.'s complaint being made public: to the reader, therefore, the falleness or justice of my observation or "reflection" must be referred; and it will answer the purpose of both Mr. B. and myself, if he will let the affair drop here; for *he* will then stand a good chance of being reputed what is his principal aim, a *diffident writer*, and I shall cease to be, what some of your readers, besides Mr. B. may possibly be disposed to think me, a *caviller*.

I am, Sir, once more,

AN OLD CORRESPONDENT.

May 17, 1805.

P. S. The mathematical portion of your readers need not be informed, that the *forgetfulness* imputed to me, respecting the assumed *approximation* substituted for the *exact ratio* of the diameter to the circumference of a circle, is a charge applying with equal propriety to every mathematician who has deduced calculations depending on the area of a circle: even Mr. Gough, whose mathematical skill is justly the admiration of thousands, whom he has never seen, and is doomed never to see, has somehow been *obliged* to be guilty of the same want of recollection, though his calculations may be considered as the result of *demonstrative truth*.

It remains now for Mr. Boswell to show the scientific world, by an example or two, how he applies his discovery to practical measurements, which it is presumed, will prove a communication of general interest.



## XV.

*Description of an accurate Method of banking the Balance of a Time-keeper. By Mr. WILLIAM HARDY. Extracted from his Letter to CHAS. TAYLOR, Esq. Sec. to the Society of Arts.\**

SIR,

Importance of the stop or banking piece used to prevent extreme vibrations in time-pieces.

THIS letter is accompanied with a drawing, a description, and a model, of a more perfect mode of banking the balance of a time-keeper, than any that has yet appeared; and its application to a time-keeper is a matter of such real importance, that the most accurate, without this most necessary appendage, is liable to such derangement, that from the most trivial cause it is in one moment rendered useless.

The author's invention is confirmed by long trial,

To preserve the good qualities of the time-keeper, on which often the strength, the wealth, the grandeur, and safety of this great empire depend, I deem it necessary that my invention should be laid before the Society of Arts, as the means of its being more generally known; and I hope that I shew proper respect to the Society, when I assure you that I do not offer any crude idea, neither could I think of giving you any trouble, until I had fully verified the utility of my contrivance by several years trial. As I can produce the testimony of some of the most eminent watchmakers in favour of my invention, I look forward with some degree of confidence, in expectation of obtaining the approbation of the Society.

and testimony.

The banking is required in watches which have a vibration through very great arcs.

It was at first imagined, that a banking to a watch with a free escapement was quite unnecessary, as the limits of banking were so great as to admit of almost twice 360, or 720 degrees; but, on trial, the balance was frequently found to exceed this quantity, and that a very slight motion given to the time-keeper (particularly when the axis of the balance became the axis of that motion), was sufficient to alter the strength and figure of the pendulum-spring, and position of the pieces in respect of the balance-wheel, so as to change the rate of the time-keeper; and, what was worse, require a

\* In their Memoirs for 1804. A premium of 30 guineas was awarded for this invention.

new adjustment of the balance, to accommodate itself to the changes made in the spring, and other parts connected with it. Hence it became necessary, that some means should be used to stop the balance at certain limits beyond its natural arch of vibration; and various attempts have been made to effect it. One way is, by a moveable piece on the axis of the balance, which banks against a pin, yet so as to suffer the balance to vibrate more than 360 degrees. Another method is to have a piece moveable on a centre in one of the arms of the balance, and applying itself as a tangent to the pendulum-spring, which passes through a hole in the piece. It has also a knee, which almost touches the plate, and just passes free of a pin placed in it. But when the balance vibrates so as to approach its utmost limits, the action of the spring, while in a state of unwinding, throws the piece outward, so as to fall in the way of the pin, and stop the balance from proceeding farther. Another mode is by a straight spring, screwed upon the plate, having a hook at the end of it, into which a pin placed in the balance strikes, when, as before, the pendulum-spring, in unwinding, touches the straight spring, and moves it a little outwards. There is also a way of banking by means of a bolt, which is thrown back by the pendulum-spring, and made to fall in the way of a pin placed in the rim of the balance. These are the principal modes of banking now in use, and they do not differ materially from one another in principle. But the weight and friction of so many pieces, on so delicate an organ as that of a pendulum-spring, are perhaps nearly as hurtful to the time-keeper as the injury it may sustain when it is left without any banking whatever.

Former methods  
of banking.

They are ob-  
jected to from  
weight and fric-  
tion.

In *Figures 1 and 2, Plate VI.* the same letters are placed, to signify the same things. A A is the balance to which the pendulum-spring is fastened in the usual way. In one of the crosses of the balance is placed a pin P, which stands a little way above its surface; and when the balance is caused to vibrate a complete circle, the pin in its motion will describe the dotted circle P O Q, and just pass clear of the inside of a projection formed on a cock B, which is fastened on the plate by means of a screw. At about one-fourth of a turn of the pendulum-spring, reckoned from its stud E, is placed a very delicate tapering piece of steel S, having a small hole in

Description,  
with reference  
to the engraving.

Description,  
with reference  
to the engraving.

it, through which the pendulum-spring passes; and it is fastened to it by means of a pin, and stands perpendicular to the curve of the spring. Let the balance be at rest, as represented in *Fig. 1*, the banking-pin at *P*, and the banking-piece at *s*. Suppose the balance is made to vibrate from *P* towards *O*, when *P* arrives at the banking-piece *s*, it will pass it without touching, because its extremity *s* lies wholly within the circle traced out by the banking-pin. But when the banking-pin *P* has arrived at *Q*, the banking-piece *s* will have advanced to *t*, by the pendulum-spring winding itself up into the figure represented by the dotted curve; and when the banking-pin *P* (now at *Q*) returns back to *P*, and passes on from *P* towards *Q*, to approach *B*, and so complete the other half-arch of its vibration, before *P* can arrive at the banking-cock *B*, the pendulum-spring will have unwound itself into the figure described by the dotted curve, and the banking-piece *s* will have advanced into the position at *r* just touching the banking-cock. Its extremity *r*, however, being thrown beyond the dotted circle, must necessarily fall in the way of the banking-pin, which arrives there almost at the same moment, and is opposed by it, without the slightest shock to the pendulum-spring. The model\* renders any farther explanation unnecessary.

WILLIAM HARDY.

No. 61, Chapel-Street, near White-Conduit-House,

Jan. 18, 1804.

## XVI.

*Description of a new Apparatus for making the gasiform Oxide of Carbon. Communicated by Mr. DEYEUX.†*

The progress of chemistry owing chiefly to improved apparatus.

IT is generally admitted, that chemistry is indebted to the invention of various kinds of apparatus, and the perfection to which they have been brought, for much of the progress it has made within these last thirty years.

Instance in that of Woulfe.

For instance, before Woulfe made known his apparatus for obtaining the aeriform fluids evolved from various substances,

\* Which is preserved in the collection of the Society.

† *Annales de Chimie*, Vol. LIII. p. 76.

either



either when exposed to the action of fire, or when brought into contact with matters capable of combining with them, the operator was obliged to employ bulky vessels, difficult to manage, and so inconvenient, that he did not wish to apply them; and when he did, it was impossible for him to collect the aeriform fluids with any accuracy; for when they were rarefied to a considerable degree, they escaped through apertures left purposely to preserve the vessels from bursting.

Inconvenience  
of the old instru-  
ments.

These inconveniences are now removed by Woulfe's apparatus, so that the operations in which gases are evolved may easily be performed in vessels of small bulk; the gases may be subjected to calculation; their quantity as well as quality ascertained with the utmost precision; and operations, which were formerly considered as very hazardous to the operator, may now be continued for hours together without the least fear of injury.

Contrasted ad-  
vantages of the  
modern arrange-  
ment of vessels.

With these advantages many others are connected; and it is known to every one, that they are owing to the degree of perfection to which chemists have brought Woulfe's apparatus, and particularly to their happy application of it on various occasions.

Yet, notwithstanding these discoveries have been carried a great way, it is more than probable that many remain to be made: too much praise therefore cannot be bestowed on those who turn their attention to this important object, since the apparatuses they invent are so many new means afforded chemists of collecting an infinite number of products which frequently escape them, and the knowledge of which may have great influence on the improvement of chemical science.

Improvements  
yet remain to be  
made.

From these motives I have thought it may be of use to make known an apparatus just invented by Mr. Baruel, operator to the chemical lectures of the Medical School at Paris.

Invention of Mr.  
Baruel;

This young chemist, who had often noticed the difficulties and even dangers incurred when it is necessary to decompose gases, or to combine them with different substances, attempted to make some alteration in the processes commonly employed in the laboratory for operations of this sort; and after several trials he invented an apparatus, which succeeded beyond his hopes.

for decomposing  
gases, or com-  
bining them with  
other substances.

I have seen this apparatus employed with the greatest success for the fabrication of the gasiform oxide of carbon. This gas, with difficulty,

Gasiform oxide  
of carbon for-  
merly procured  
gas, with difficulty,

is now easily made.

gas, which formerly was to be procured only with difficulty and in small quantity, may now be obtained easily, readily, and without much expense, which affords the advantage of subjecting it to many more experiments than has hitherto been done.

Other uses of the apparatus.

The apparatus in question may be of use likewise for the preparation of sulphurated hydrogen gas, carbonated hydrogen gas, and phosphorated hydrogen gas: it may likewise be employed for saturating a gas with any substance whatever, particularly when a high temperature is requisite for this saturation.

The better to make known the apparatus of Mr. Baruel, I will give the description of it communicated to me by the author; and add a sketch of it, which will give a more complete idea of all its parts. *Plate VIII.*

Description of the apparatus, and method of procuring gasiform oxide of carbon.

Suppose it is required to make gasiform oxide of carbon: some charcoal is to be taken very dry and carefully chosen, broken into small pieces, and introduced into the three gun-barrels, B, C, D. The charcoal is to be pressed lightly together with an iron rod, so as to occupy only the part of each barrel that is to be heated, but it must not be rammed hard. The three gun-barrels are then to be placed horizontally side by side in a reverberatory furnace, A, leaving about two inches distance between them; secured in their places with moistened brick earth, and covered with the dome of the furnace.

This done, fix in the mouth of the barrel B the glass tube E, which is curved so as to admit its other extremity to be inserted into the neck of the bottle F; the neck of this bottle being large enough to hold likewise the pipe of the curved funnel G. Into the opposite end of the barrel B one of the ends of the curved tube H is to be introduced, the other end of the tube being inserted into the opening of the barrel D, so as to form a communication between B and D. A similar tube I is fitted to the other extremity of D and the adjacent end of C, forming a communication between these two barrels. Lastly, From the opposite extremity of the barrel C issues the tube K, which is bent at right angles, so that its second curvature passes under the receiver M, placed on the shelf of the pneumatic tub with water L.

Every

Every thing being thus arranged, carbonate of lime diluted with a small quantity of water is to be poured into the bottle F, and after all the joints of the tubes have been luted with great care, a fire is to be kindled in the furnace. As soon as this fire is sufficiently strong to make the gun-barrels red-hot, sulphuric acid is to be poured into the funnel G, and, when it comes into contact with the carbonate of lime in the bottle F, it will expel a large quantity of carbonic acid. This acid presently passes through the tube E into the barrel B, is conveyed from B to D through the tube H, from D to C through the tube I, and thence issuing by the tube K, comes out beneath the receiver M, placed on the shelf of the pneumatic tub.

Description of the apparatus, and method of procuring gasiform oxide of carbon.

The object proposed by Mr. Baruel in this arrangement of his apparatus, was to oblige the carbonic acid gas evolved from the carbonate of lime, to traverse the charcoal contained in the three gun-barrels, and thus saturate itself with all the carbon it could take up.

In fact it is easy to conceive, that this method must be more certain and expeditious than that formerly employed, when the operator was satisfied with passing the gas through a single barrel. It is true it was collected, and subjected to a second operation, or even to a third; but this mode was tedious, and much of the gas was always lost. In this new method on the contrary nothing is lost, and a product is separated at once, which possesses all the properties that characterize the gasiform oxide of carbon, and which may be used unsparingly, since it is always obtainable in large quantity.

This method more certain and expeditious than the old;

for nothing is lost, and the product is obtained at once.

## XVII.

*Description of an improved Mill for levigating Painters Colours.*

*By Mr. JAMES RAWLINSON, of Derby.\**

THE hitherto very unmechanical, inconvenient, and highly injurious method of grinding poisonous and noxious colours, led me first to imagine a better might easily be contrived for that purpose. It must be obvious to every person, that the

Great inconveniences and unwholesome effects of grinding colours on the stone.

\* From the Memoirs of the Society of Arts for 1804, who awarded him the silver medal.

method



method hitherto adopted of grinding colours on an horizontal marble slab, with a small pebble muller, requires the body of the person who grinds to bend over that slab, and consequently his head; which causes him constantly to inhale the noxious and poisonous volatile parts of the paint, which is not unfrequently ground with oil saturated with litharge of lead; and if we may judge from the very unhealthy appearance of these men, accustomed to much colour-grinding, it should seem the bad effects of this employment require a speedy remedy.

Machine by which the work is much better performed, and without inconvenience.

The machine, of which I now send the Society a model, has not only the advantage of being an effectual remedy of this extensive and severe evil to recommend it, but it grinds the colour much easier, much finer, and much quicker, than any method hitherto adopted. Having occasion for a considerable quantity of colour-grinding in the profession in which I am engaged, and that in the finest state possible, and having made use of this machine for several years, and being more and more convinced of its utility, I thought it my duty to present it to the Society of Arts, hoping that it might not be altogether unworthy of their attention. The roller of the machine that I use is sixteen inches and a half in diameter, and four inches and a half in breadth. The concave muller that it works against covers one-third of that roller: it is therefore evident, that with this machine I have seventy-two square inches of the concave marble muller in constant work on the paint, and that I can bring the paint much oftener under this muller in a given space of time, than I could by the usual method with the pebble muller, which is seldom more than four inches in diameter, and consequently has scarcely sixteen square inches at work on the paint, when my concave muller has seventy-two. I do not mean to say that a roller, the size of that which I now use, is the largest which might be employed; for truly I believe that a roller two feet in diameter, with a concave muller in proportion, would not be hard work for a man; and then the advantage to the public would be still farther increased.

It works with five times the surface of the muller,

and is applicable to water-colours as well as those in oil.

This machine will be found equally useful for the colours ground in water, as for those ground in oils; and I doubt not but the great importance of this simple machine will be very soon generally experienced in all manufactories where colours are used. The labour necessary with this machine, in grind-

ing

ing colours exceedingly fine, is very easy. It is useless to enter into any minute description in this place, as a bare inspection of the machine must sufficiently explain itself.

To the colourman it would evidently be an essential saving of labour, and consequently of expense, which will probably have some weight as a recommendation; and the advantages to the colour-grinder have been already stated.

*Plate V. Fig. 3.* A is a roller or cylinder made of any kind of marble; black marble is esteemed the best, because it is the hardest, and takes the best polish. B is a concave muller covering one-third of the roller, of the same kind of marble, and fixed in a wooden frame *b*, which is hung to the frame E at *ii*. C is a strong piece of iron, about an inch broad, to keep the muller steady, and is fixed to the frame with a joint at *f*. The small binding-screw, with the fly nut, that passes through the centre of the iron-plate at *c*, is for the purpose of laying more pressure on the muller, if required, as well as to keep it steady. D is a taker-off, made of a clock-spring about half an inch broad, and fixed in the manner of a frame-saw in an iron frame *k*, in an inclined position to the roller, and turning on pivots at *d d*. G is a slide-board to draw out occasionally, to clean, &c. if any particles of paint should fall from the roller, and which also forms itself for the plate H, to catch the colour on as it falls from the taker-off. F is a drawer, for the purpose of containing curriers shavings, which are the best things for cleaning paint-mills.—E is the frame.

Description, by reference to the drawing.

Previous to the colour being applied to the mill, I should recommend it to be finely pulverized in a mortar, covered in the manner of the chemists when they levigate poisonous drugs\*, or dry grinding, previous to levigation. This process of dry-grinding is equally necessary for the marble slab now in use; after which it should be mixed with oil or

\* Or rather in an improved mill, used at Manchester by Mr. Charles Taylor, for grinding indigo in a dry state, of which I have annexed a drawing, and reference, to render the whole business of colour-grinding complete.—*Note of the Author.*

This is the same apparatus as was used under the name of a *philosophical mill*, in the laboratory at Gottorp, about the beginning of the last century. See the memoir of Dr. Joel Langelot, with an engraving, in Lowthorp's Abridgment of the Philosophical Transactions, III. 318.—W. N.

water,

Method of  
working.

water, and with a spatula or pallet-knife put on the roller, near to the top of the concave muller, and the roller turned round, which takes the colour under the muller without any difficulty, and very few turns of the roller spread it equally over its surface. When it is perceived sufficiently fine for the purpose required, it is very easily taken off by means of the taker-off described, which must be held against the roller, and the roller turned the reverse way, which cleans it very quick and very completely; and the muller will only require to be cleaned when you desist or change the colour. It is then turned back, being hung on pivots to the frame at *z z*, and cleaned with a pallet-knife or spatula very conveniently. Afterwards, a handful of curriers shavings held on the roller, with two or three revolutions cleans it effectually; and there is less waste with this machine than with any marble slab.

Quantity and  
fineness of  
grinding.

As to the quantity ground at once on this mill, it must be regulated by the state of fineness to which it is required to be ground. If it is wanted to be very fine, a smaller quantity must be put on the roller at a time; and as to time requisite for grinding a given quantity of colour, this will also depend on the state of fineness to which it is ground. I have observed that my colour-grinder has ground the quantity of colour which used to serve him per day, with this machine, in three hours, and, as he said, with ease. The colour also was much more to my satisfaction than in the former way, and attended with less waste.

I have mentioned the pulverizing the colours in a covered mortar, which would prevent waste, and prevent the dust and finest parts of noxious colours from being injurious to the grinder. In some manufactories, where large quantities of colours, prepared from lead, copper, and arsenic, are used, this precaution is particularly necessary. I do not mean to say that my machine is intended to supersede the paint-mill now in use for coarse common colours. It is intended for no such purpose; but to supersede the use of the very awkward and unmechanical marble slab now in use, and on which all the colours for china manufactories, coach-painters, japanners, and colour-manufacturers for artists, &c. &c. are now ground.

This mill is not  
a crude project,  
but has been  
used several  
years.

Several of the colour-manufacturers have expressed to me their great want of such a machine; and that I had no desire of troubling the public with a machine that would not answer,

is



is evident, from my having used it several years before I presumed to recommend it to their attention. Being therefore now completely convinced of its utility, and hoping that it might relieve a number of my fellow-creatures from a dangerous employment, I have ventured to commit it to the protection of the Society of Arts; hoping, through their means, to see its ultimate success. And, farther to give the Society the most complete assurance in my power, I have annexed the opinion of a very ingenious and mechanical friend of mine who has frequently seen it work. If any other questions should occur to the Committee, that may be in my power to explain, I shall gladly do so.

I am, Sir,

Your most obedient servant

JAMES RAWLINSON.

Derby, Feb. 6, 1804.

Charles Taylor, Esq.

P. S. When the colour is ground, I recommend the following mode of tying it up in bladders, in preference to the usual method. Instead of drawing the neck of the bladder close, in the act of tying it, insert a slender cylindrical stick, and bind the bladder close around it. This, when dry, will form a tube or pipe, through which, when the stick is withdrawn, the colour may be squeezed as wanted; and the neck again closed by replacing the stick. This is not only a neater and much more cleanly mode than the usual one of perforating the bladder; and stopping the hole with a nail, or more commonly leaving it open, to the prejudice of the colour; but the bladder, being uninjured, may be used repeatedly for fresh quantities of colour.

N. B. The barrel of a quill may be tied, in place of the stick, into the neck of the bladder, with its closed end outwards, which will keep the colour secure in travelling, and when used, the end of the quill being cut off, it may afterwards be closed by a stick.\*

\* A certificate from Mr. Thomas Swanwick, of Derby, and also from Mr. John Middleton, of St. Martin's Lane, confirming the above statement, accompanied these papers.

Reference

*Reference to the improved Mill for grinding Indigo, or other dry Colours.*

Description of the mill for dry grinding. *Plate V. Fig. 4.* L represents a mortar made of marble or hard stone; one made in the common way will answer. M,

A muller or grinder, nearly in the form of a pear, in the upper part of which an iron axis is firmly fixed, which axis, at the parts N N, turns in grooves or flits, cut in two pieces of oak projecting horizontally from a wall, and when the axis is at work, are secured in the grooves by iron pins, O O. P, the handle, which forms a part of the axis, and by which the grinder is worked. Q, the wall in which the oak pieces N N are fixed. R, a weight which may occasionally be added, if more power is wanted.

*Fig. 5.* shews the muller or grinder, with its axis separate from the other machinery; its bottom should be made to fit the mortar. S is a groove cut through the stone.

On grinding indigo, or such substance, in a dry state, in this mill, the muller being placed in the mortar, and secured in the oak pieces by the pins, the indigo to be ground is thrown above the muller into the mortar; on turning the handle of the axis, the indigo in lumps falls into the groove cut through the muller, and is from thence drawn under the action of the muller, and propelled to its outer edge within the mortar, from whence the coarser particles again fall into the groove of the muller, and are again ground under it; which operation is continued, till the whole of it is ground to an impalpable powder; the muller is then easily removed, and the colour taken out.

A wood cover, in two halves, with a hole for the axis, is usually placed upon the mortar, during the operation, to prevent any loss to the colour, or bad effect to the operator.

## XVIII.

*A new and cheap Method of purifying Gold and Silver. By  
ANDREW THOMSON, Esq. of Banchory, near Aberdeen.  
In a Letter from the Discoverer.*

To Mr. NICHOLSON.

SIR,

I INTENDED to have deferred the present communication till such time as I should have it in my power to lay before the public the complete series of experiments in which I have been engaged with regard to the purification of gold and silver. But unluckily I mentioned a few particular circumstances with regard to them, to a man who took it upon him, without my knowledge, to send an account of them for publication to a periodical work. As I understand that work will not appear so soon as your next number, I beg, if you think it worthy of a place, that you will insert the following account of some attempts I have been making to purify the precious metals.

This publication is prematurely made, because the inventor might else be anticipated.

Being much at a loss for want of a crucible of pure silver for the analysis of some minerals, and as all the usual methods practiced for purifying that metal are very troublesome, I set myself to consider the various operations on metals, in hopes of falling on a more simple way of accomplishing my purpose. At length, I found a process of Pelletier's, which promised to succeed, and mine is merely extending his idea a little further than he did himself.

Process of Pelletier for extracting copper from bell-metal.

He was, I believe, employed by the French government to discover an easy way of separating the tin from copper on bell-metal, and the process he gave, is this. Upon the melted bell-metal project black oxide of manganese in powder, frequently stirring the metal till all the tin becomes oxidated by the manganese. He adds a caution, not to add too much manganese, otherwise part of the copper also will be destroyed.

He oxides the tin by manganese;

in such proportion as not to affect the copper.

It immediately struck me, that in this way I might be able to oxidate the copper which alloys our silver, and upon making the trial I succeeded completely; I had some impure silver rolled out to about the thickness of a shilling, this I coiled up; then I bedded in manganese; and

The author's improved process. Coarse silver was rolled out; then coiled up; bedded in manganese; and



strongly ignited for a quarter of an hour.

All the metal was oxidized.

coiled up spirally, and put into a crucible, the bottom of which was covered with black oxide of manganese. I then added more oxide till the silver was covered, and all the space between the coils completely filled. A cover was then luted to the crucible, and a small hole left for the escape of oxygen gas. When this had been exposed for a quarter of an hour to a heat sufficient to melt silver, I found the surface of the manganese brown from the loss of oxygen; but, where the silver had been, the whole was one uniform black powder, without the least appearance of metallic lustre, so that I had no doubt, that even the silver was become an oxide.

The whole contents were then put into a larger crucible with thrice its bulk of green glass.

I then put the whole contents of the first crucible into a second of a larger size, into the bottom of which I put a quantity of pounded green glass, about three times the bulk of the contents of the first crucible, and luted on a cover as before, to prevent the access of any inflammable substance.

Strong heat fused the glass, and reduced the silver pure, and alone in a button.

The crucible was then exposed to a heat sufficiently strong to melt the glass very fluid. Upon cooling and breaking the crucible, I found the silver at the bottom perfectly pure, as its oxide alone could part from its oxygen without the access of some inflammable substance. I find this process answers equally well for purifying gold, and to me it seems to possess some advantages over all the former methods. The materials used are cheap, and a large quantity can be refined as soon, and as easily as a small quantity, by merely altering the capacity of the crucible you use.

This process answers equally with gold.

The metal must be in thin or small masses.

I tried the same operation on gold and silver in round masses, but found it went on very slowly, and what I scarcely expected, in the first part of the process of oxidating the metals, the remaining metal continued uniformly impure or nearly so, until the whole was oxidated.

The proportions, &c. are not here given, because the author has hastened to communicate his process.

I regret that I have been forced to make this matter public, before I could do it in a manner satisfactory to myself. I wished to have given the exact proportions of alloy, manganese, and glass to be generally used, and to have ascertained if there is any truth in the old opinion, that saltpetre melted with gold destroys a part of it. I suppose that idea may have arisen from the oxygen given out by the nitre in a high heat, oxygenating the copper contained in the impure gold, which has been the subject of the experiment.

Since the above was written, I have been informed that He vindicates this matter has actually been published, but know not in what his claim.

I hope you will still have the goodness to insert this as an original communication, as I do not think the person who has published it will have the impudence to call it his own, and as Mr. Kirwan, and other celebrated chemists long ago advised me to publish it, I have already stated my reasons for not following such good advice.

As I have now been forced to appear before the public, I have hopes I shall be able to prevail on some of my friends to commit themselves in the same way, in the confidence that their labours will be found useful to the public.

I am, Sir,  
Your's truly,

ANDREW THOMSON.

Banchory, by Aberdeen,

May 5th, 1805.

## XIX.

*Memoir on the Propagation of Sound. By M. HASSENFRATZ.\**

THE production of sound is ascribed by all natural philosophers to the vibration of the molecules of bodies. Sound produced by vibrations,

The vibration of these molecules admits of two kinds of modification; 1st, in velocity; 2ly, in magnitude. The first of these determines the nature of tones; the second, their force or intensity. which differ in velocity and in magnitude.

Sound is transmitted to the ear by the molecules that fill the medium or interval between the sonorous body and the organ of the hearing. The movement of the sonorous body impresses on the molecules of the medium an impulse, which they transmit from one to another, till it reaches the ear, with a greater or less velocity. In this transmission the vibration may undergo two kinds of alteration: 1st, in its velocity; 2dly, in its intensity. In this memoir I shall only transcribe some experiments relative to the velocity of sound. The sonorous body impresses its vibrations on some medium, the vibrations of which are continued to the ear.

Philosophers have long been engaged in determining the velocity of sound, but considering the air as the chief medium The velocity of sound hitherto examined only in air.

\* Annales de Chimie, Vol. LIII. p. 64.

by



by which it is transmitted to the ear, they have attended only to its velocity in air, and have employed two different methods to determine this, theory and experiment.

This velocity uniform in a given medium,

and the density of the medium is one of its elements.

Not affected by the height of the barometer.

Blanconi asserts it to be less in winter than in summer.

Derham denies this, but is probably wrong.

Not affected by rain or fine weather.

These two methods have led to the following remarkable results: 1st, that the velocity of sound in a given medium, is uniform, whatever its distance from the phonic centre, and whatever its intensity: 2dly, that the density of the medium at equal pressures is one of the elements of the velocity; for it has been found by theory, that the velocity of sound is the same as that of a body falling from half the height of an atmosphere supposed to be of equal density with the air in the place where the sound is transmitted; and by experiment, that, all other circumstances being equal, the velocity of sound is the same at different pressures of the barometer; so that it is equal on the summit of a mountain and on the sea-shore. In fact, the density of the air being proportional to the compressing weight, the height of the column of mercury in the barometer, divided by the density occasioned by this pressure, is a constant quantity; and the height of the atmosphere of a uniform density being equal to the total weight of the air divided by its density, it follows that the height of the barometer ought to make no difference in the velocity of sound. Blanconi asserts (Comment. Bonon, vol. II. p. 365), that the velocity of sound is less in winter than in summer, since, according to his experiments, it takes four seconds more in winter to traverse a space of sixteen Italian miles. Derham affirms, that the velocity of sound is the same whether the air be extremely hot or extremely cold, though his tables of experiments will be found on examination favourable to the opinion of Blanconi; for the greatest velocity of sound in them was on the 5th of April, at one o'clock in the afternoon, being three miles in 111 half-seconds, and the least velocity on the 12th of February, at six o'clock in the evening, being three miles in 122 half-seconds. As the experiments on the velocity of sound undertaken by the Academy of Sciences in 1737 were made at temperatures that exhibit only two or three degrees difference, perhaps it would be well, as Mr. Laplace thinks, if they were repeated at a time of the year when the temperature is very different; for experience has taught us, that this velocity is equal in rainy and in fine weather, so that nothing but change of temperature can produce any variation in this respect.

Wha



Whatever these results may prove, as the experiments on the velocity and propagation of sound have been hitherto made in the air alone, it was an interesting enquiry to determine the velocity of sound, when transmitted by other bodies, and particularly of different densities with respect to air. Mr. Laplace, to whom branches of physical science are indebted for improvement, invited me about eight months ago to make experiments on this subject, and particularly on the propagation of sound through solid bodies; and the experiments of which I shall give an account in this paper were principally made in consequence of that gentleman's suggestion.

Making experiments in the quarries beneath Paris on the transmission of sound through long galleries, I caused a person to strike with a hammer against a mass of stone, retiring at the same time by degrees from the place of striking, in order to distinguish if possible the sound transmitted by the stone from that transmitted through the air. Placing my ear against the mass of calcareous stone through which these galleries are carried, at a short distance I distinguished two sounds perfectly separate, one transmitted by the air, the other by the stone. Both sounds grew weaker in proportion as I retired from the striking point; but that transmitted by the stone was weakened much more rapidly than that transmitted through the air. In a gallery excavated beneath Rue de la Harpe the sound transmitted by the stone ceased to be audible at 134 paces distance; and in a gallery beneath Rue de St. Jaques at 140 paces. Through the air the sound was transmitted to 400 or 440 paces distance. The sound transmitted by the stone always reached the ear much sooner than that transmitted by the air.

Experiments on the transmission of sound through the stone in quarries.

It was conveyed through the stone only 140 paces; — but through the air 440. It passes quickest through the stone.

Mr. Berthollet, to whom Mr. Laplace imparted these results, desirous of being assured whether the sound of a hammer could be transmitted through a mass of stone 140 paces thick, requested Mr. Gay, by my desire, to be present at my experiments. With this young chemist I repeated the experiment of the transmission of sound through stone on several separate masses, and he convinced himself, that sound was capable of being transmitted through a mass 150 paces in length.

Experiment repeated, and

the sound conveyed 150 paces.

It was long ago observed in working mines, that the noise was propagated to a very great distance through masses of rock; and the line of the sound heard through the stone serves on many occasions to determine the direction in which the gal-

The propagation of sound through stone long ago observed by mechanics.

Its velocity apparently equal to that of light.

The differences of the distances to which sound is transmitted through solid masses remarkable.

Experiments repeated,

on timber.

Conveyed farther than in the open air.

Its velocity apparently equal to that of light.

eries are carried on; but no one had attempted to observe, whether the velocity of the sound transmitted by the stone differed from that of the sound transmitted by the air. My experiments in the quarries underneath Paris have taught me, that the difference is considerable; and when the gallery is sufficiently straight, to be able to discern the motion of the hammer with the eye, no calculable difference can be perceived between the conveyance of the motion to the eye and that of the sound to the ear.

The distance which the sound of the hammer can be conveyed to the ear varies considerably with the nature of the stone and the separations or fissures in the mass. Having caused a man to strike with reiterated blows against an isolated wall, built of common stone of the same kind as that in the quarries, and cemented with mortar, the sound was transmitted only thirty paces. Striking in the same manner on a parapet of hewn stone raised on the borders of the Seine, the sound was transmitted 46 paces. These experiments were made in the open air by day, consequently under circumstances less favourable to the propagation of sound than when on the calcareous masses in the quarries; but the difference between 30 and 46 paces, under the same circumstances, on masses differing only in the dimensions of the stones of which they were formed, is very remarkable.

Encouraged by the success of my experiments in the quarries, and by the invitation of Mr. Laplace, I attempted to repeat the same experiments on different substances.

By the side of the high road that leads from the place *de la Concorde* to *Chaillot* along the bank of the Seine, on the stone wharf of St. Leir, opposite the steam-engine of Gros-Caillois, is placed a railing 210 paces in length, formed of 31 pieces of timber, separated by four large posts. The blow of a hammer at one extremity of this railing was heard distinctly at the other, though through the air it was audible only 120 paces. At the distance at which both the sounds were audible, that through the wood was heard long before the other; and when, standing at the greatest distance from the place of the blow, I heard only the sound transmitted through the timber, the velocity of its transmission was so great, that it was difficult to distinguish any interval between the perception of the sound by the ear, and of the motion of the hammer by the eye.

Having



Having convinced myself, that the propagation of sound through stone and through wood was effected with much greater velocity than through air, and that the time of its transmission to such short distances as those on which I was able to make experiments was too little for calculation, I was desirous of knowing whether the velocity of its transmission through metallic substances were the same.

Several experiments on bars of iron fixed on solid masses, as the bars that hold together the stones of parapets, having given me uncertain results, I sought for isolated bars of sufficient length to afford some certainty. My first experiment was made on the upper bars of an iron railing, 34 paces long, erected on one of the walls of the garden of the Legislative Body, adjoining to the Place des Invalides. On striking one extremity of this assemblage of bars, two distinct sounds were heard at the other end; that transmitted by the bars, and that by the air; the former being always heard first. The same experiment afterwards repeated on bars of different lengths, gave me the same result; and this result is such, that it is impossible to distinguish, at the small distances at which these experiments were made, any difference between the transmission of the motion by light, and that of the sound by the solid medium.

Repeating my experiments on the velocity and propagation of sound through the masses of stone in the quarries beneath Paris, in company with Mr. Gay, this young chemist imagined that he distinguished two sounds transmitted through the air, one grave and the other acute, which reached his ear in succession, the graver sound appearing to have the greater velocity.

Similar experiments and results with long bars of iron.

Mr. Gay supposed he heard two sounds through the air, first a grave, then an acute.

This result, though contrary to the theory of the propagation of sound, according to which grave and acute sounds ought to have the same velocity, had already been conjectured by several philosophers, particularly by Mairan, and was therefore worth confirmation. For this purpose I stretched two strings, one of brass, the other of catgut, so as to make them emit two different tones, the first one more acute, the second one more grave; then striking both these strings at once with the wood of a black lead pencil, the two sounds, which were confounded together at first, appeared to separate at the distance of 400 paces in a large gallery of the quarries, and we both imagined we could distinguish the graver sound first.

Mairan and others had conjectured the same.

Experiment to determine this.



Probable cause  
of fallacy in this  
experiment.

While observing the propagation of sound in the galleries, I had several times occasion to remark, that a sound at a great distance was frequently repeated, either by reflection or by the vibration of the walls, so as to cause two different sounds to be heard and distinguished, which reached the ear in succession. As it was possible that the difference which appeared to Mr. Gay and me might have been produced by the cause here mentioned, I determined to repeat the experiment in the open air.

The experiment  
repeated in the  
open air.

For this purpose I took two glass bells, the tones of which were as an octave and a fifth, that is, the ratio of their vibration was as 1 to 3. A hammer was so adjusted as to strike both the bells at once, and make them sound at the same instant. Carrying this instrument into the fields, I endeavoured to ascertain whether the two sounds reached the ear in unequal times. Several experiments repeated in various places, made me believe a long time, that their velocity was unequal; but having observed, that on some occasions the sounds reached the ear at the same time, I was led to remark, that whenever I imagined I distinguished a difference of velocity, this difference had been occasioned by a repetition of the sound, and that frequently very trifling obstacles, as trees or hedges, were sufficient to produce this repetition. I repeated my experiments therefore anew in the midst of plains of greater or less extent, as those of Montronge, Grenelle, St. Denis, &c. and whenever I was remote from any obstacle capable of producing a repetition, both sounds were heard at the same time. With my instrument I could distinguish the sound of the two bells at the distance of 700 paces or 631 yards; whence it follows, that both experiment and theory concur to demonstrate, that grave and acute sounds have the same velocity.

The double  
sound produced  
even here by an  
echo.

Grave and acute  
sounds have the  
same velocity.

Conclusions from  
the experiments.

From the experiments related in this paper it follows: 1st, that the velocity of sound differs according to the medium by which it is propagated: 2dly, that this velocity is much more considerable, when it is propagated by solid and very dense bodies, than when by aeriform bodies, and of little density: 3dly, that both grave and acute sounds have the same velocity; a result to which theory led us.

## XX.

*A very advantageous Mode of preparing Muriate of Barytes by the Mutual Decomposition of sulphurated Barytes and Muriate of Lime. By Mr. GOETTLING.\**

MR. DRIESSEN of Groningen first observed, that muriate of lime and sulphate of barytes decompose each other at a high degree of heat. Trommsdorff verified the experiment of the learned Dutch professor, and applied the principle to the preparation of muriate of barytes. He perceived, however, that the whole of the sulphate of barytes was not decomposed in this way, and he thought, that equal parts of the two salts were the suitable proportion for decomposing them as completely as possible. Mr. Goettling has deemed the subject worth farther inquiry, the result of which was the process I shall now describe.

Driessen first decomposed sulphate of barytes by muriate of lime; and next Trommsdorff.

One part of native sulphate of barytes in fine powder is to be mixed with half a part of muriate of lime.† This mixture is to be introduced into a Hessian crucible, which is to be closely covered, and brought gradually to a red heat. The matter must be kept in a state of incandescence a full half-hour, and frequently stirred. It is then to be poured out into an iron cone, and after being coarsely powdered, is to be thrown into three parts of boiling water. The vessel being immediately taken from the fire, the mixture is to be stirred occasionally with a glass spatula, and the undissolved matter is then to be left to subside. The clear liquor being decanted off, the residuum is to be poured into a filter of linen of a close texture, and the fluid lightly pressed out. The residuum being again lixiviated with one part of water, is to be strained as before. The liquors are then to be mixed together, and evaporated to a pellicle, to obtain the salt by crystallization. In this way we shall have five-eighths of a part of muriate of barytes. The

Mr. Goettling's process for preparing muriate of barytes in this way.

\* Van Mons's *Journal de Chimie*, Vol. VI. p. 92. Abridged from the *Taschen-Buch fuer Scheidekuenstler*.

† This muriate is obtained in abundance in our laboratories, as an adventitious product: it may be procured likewise at a very trifling expence by adding lime to the mother-water left after refining common salt.

Muriate of lime, how obtained.

mother-

mother-water of the first crystallization, which is almost wholly muriate of lime, is to be set aside for a fresh operation, or for any other purpose.

As the insoluble mass of sulphate of lime still contains a large quantity of sulphate of barytes, indeed about half the original quantity, it is to be treated afresh as above, with one fourth part of muriate of lime; and the same process is to be repeated with an eighth part of the same salt. Thus we obtain an addition from one to two eighth parts of muriate of barytes. The salts of the various crystallizations require to be dissolved and re-crystallized anew, in order to free them from a little muriate of lime, which adheres to them in the first crystallization.

The salt to be purified by re-crystallization.

## XXI.

*Observations on the Rectification of Nitric Acid, by Mr. STEIN-ACHER, Druggist at Paris.\**

Nitric acid contaminated in its first portions with muriatic. Why, according to Berthollet.

IT has long been known, that the first portions of nitric acid distilled from litharge contain muriatic acid. Berthollet explains this phenomenon by saying, that the oxide of lead dividing its action between the two acids, both are subjected to the action of expansibility produced by the heat. Messrs. Welter and Bonjour assert, that, if muriate of silver be employed, oxygenated muriatic acid is formed, which rises with the first portions. If I may be allowed to give the results of my labours after those of so many able chemists, I would say, that, when the nitric acid has been sufficiently concentrated before being subjected to rectification on silver, or on oxide of lead, the first part of the rectified acid is found on trial to contain no muriatic acid, notwithstanding the nitric acid contained much of it after its concentration; and hence I infer, that an excess of water is the true cause, that diminishes the attraction of the muriatic acid for the oxide of lead or of silver.

True reason is the presence of an excess of water.

Concentrating the acid insufficient, if the proportion of litharge be improper, and the distillation carried to dryness.

The operator however would in vain expect to succeed by merely concentrating his acid before rectifying it, if he used a determinate proportion of litharge, or distilled to dryness, as

\* Van Mons's *Journal de Chimie*, Vol. VI. p. 88.

several



several authors direct. The quantity of litharge must vary from one to eight sixteenths of the weight of the acid, according to its degree of impurity. On the other hand, if distilled to dryness, the latter portions of the nitric acid will carry over with them in solution muriate of lead, or of silver.

Four kilogrammes (10lb. 8oz. 16dwt. troy) of nitric acid of the shops, at  $35^{\circ}$ , containing muriatic acid, and a very little sulphuric, are first distilled in a reverberatory furnace in a retort placed on an earthen vessel filled with sand. The fire must be so regulated that the drops succeed each other slowly, and half the acid is to be thus drawn off. It will then give  $15^{\circ}$  of Baumé's areometer. What remains in the retort is to be poured into a bottle. Its specific gravity will be expressed by  $4^{\circ}$  of the areometer. Litharge being thrown into it in fine powder, and stirred with a glass rod, will be converted into a white powder in a few hours. More litharge is then to be added in the same manner; and this is to be continued, till the litharge retains its colour after several hours standing. The muriate and sulphate of lead are then to be left to subside entirely, and the acid is to be decanted off into a tubulated glass retort, placed on a small earthen plate filled with sand, in the midst of a reverberatory furnace, all the parts of which are retained except the dome. A receiver is to be adapted, which fits closely without luting; for, as the vapour of the acid easily destroys every kind of lute, the product would otherwise be liable to become impure; and the distillation is to be so conducted, as to admit a short interval between the fall of each drop. Great care must be taken not to suffer the acid to boil, for thus it would be dissipated in incompressible vapour. The first half that comes over marks  $35^{\circ}$ , the second  $40^{\circ}$ . Both portions are colourless, and have all the properties of a very pure nitric acid, if  $\frac{1}{32}$  of the liquor be left in the retort.

Mr. Steinacher's process.

If a stop be put to the distillation after the first portion is separated, and the retort left to grow cold, you will obtain a beautiful crystallization of muriate of lead in large and very brilliant striated hexaedral laminæ. This salt is a true muriate, for sulphuric acid expels from it vapours easily distinguishable to be those of the muriatic acid. On continuing the distillation, these crystals gradually lose their regular figure, and at length fall to the bottom in a powdery precipitate.

As to seeing a beautiful crystallization of crystals of muriate of lead, may be obtained when half the acid is distilled off.

## XXII.

*Easy Method of making the very combustible Oxide of Phosphorus.*  
By AMICUS.

To Mr. NICHOLSON.

DEAR SIR,

Oxide of phosphorus.

YOU know very well that phosphorus united to a much smaller proportion of oxygen than is requisite to render it into the acid state, brings it into the condition of an oxide which sets on fire sulphur, on just rubbing it against a common match. But the common method of oxidizing phosphorus for phosphoric matches by fixing it in the bottles with a hot iron, is troublesome and wasteful. This oxidation, however, may be effected with great facility and economy by exposing a large proportion, viz. a hundred grains of phosphorus in a jar containing half a pint measure of oxy-muriatic acid, in which circumstance the phosphorus will be melted and fume, but scarcely take fire. After cooling, it must be kept excluded from the air, to prevent the inflammation from mere exposure.

DEAR SIR, Yours,

AMICUS.

May 28th, 1805.

## XXIII.

*Description of an extremely sensible Micro-electrometer.* By Mr. MARECHAUX.†

A piece of leaf silver is suspended so as to be moveable in a glass cylinder.

IN a glass cylinder, about an inch and half in diameter and five or six inches high; a piece of leaf silver is suspended from a small pair of nippers, capable of being lowered or elevated as the length of the leaf may require. The piece that carries the nippers may likewise be moved horizontally, so that the leaf may be moved at pleasure nearer to or further from a sphere of copper, which is one of the poles of the instrument.

\* Translated from Von Mons's *Journal de Chimie*, Vol. VI. p. 38. Abridged by Van Mons from Gilbert's *Annalen der Physik*.

The glass cylinder has, about 1" (centimetre, near 4 lines English measure?) from the plate on which it is fixed, a small hole, through which passes the extremity of a micrometer-screw, about the size of a large goose-quill, and very carefully cut. This screw has fifty threads in a Rhyndland inch, cut very deep, though very fine. It is made of two pieces, and should be at least three quarters of an inch long, to avoid any shake. The extremity of this screw carries a little ball, which is put on after the screw is passed through the opening in the cylinder. To avoid all friction against the glass, care is taken that the screw, when turned, does not touch the edges of the cylinder. The screw carries a plate three inches and a half (3.8 English) in diameter, which has 360 divisions, and consequently divides each thread of the screw into as many parts.

Through a hole in the cylinder a micrometer-screw passes,

In this manner we are enabled to determine the sphere of activity of the two electricities in 18000ths of a Rhyndland inch. The mounting which contains the female screw has a small pillar, which advances on the plate, and carries an index, by which the degrees are marked with precision.

by which the distance of  $\frac{1}{18000}$  of an inch may be measured.

To use this instrument, which is perfectly insulated by the glass plate on which it rests, the first thing is to place the plate in such a position that the 0 shall be exactly under the index. The adjusting screw which carries the nippers is then to be moved till the leaf silver is so near the ball, that no light passes between them. Thus we have the point of contact, and of 0 for the sphere of activity of the two electricities. To be certain that the leaf is brought as close as possible to the ball without being forced out of the vertical direction, the micrometer screw should be moved a turn first backward then forward several times, and the position of the leaf observed every time the ball is brought into contact with it. The instrument being thus adjusted, the micrometer-screw must be moved backward one turn, and then we have between the leaf and the ball a distance of one-fiftieth of an inch, which may be subdivided at pleasure by means of the plate; for with a plate near four inches in diameter, and by means of the fine needle on which it turns, we may distinguish half or even a quarter of a degree if necessary.

Mode of preparing the instrument for use.

An adjustment fixed to the plate on which the cylinder rests serves to ascertain whether the leaf of silver be in fact drawn out of the leaf.

Apparatus for ascertaining the perpendicularity of the leaf.



out of the vertical line by any attractive power. This consists of a fine silk thread, stretched in the same plane as the silver leaf. By means of the screw this thread may be moved both horizontally and vertically, so as to follow the movements of the silver leaf.

Mode of using  
the instrument.

Every thing being thus arranged, make a communication by means of conducting wires between a single couple of metallic disks, or one constituent part of a pile, placed on a plate of glass, and the instrument, so that one of the metals shall communicate with the top of the instrument, and the other with the bottom. Then by means of a glass handle fitted to it move the plate slowly from one degree to another, and you will find the leaf touch the ball with ordinary electricity, when it is  $60^{\circ}$  or  $80^{\circ}$  of the micrometer screw from the vertical plane, in which the leaf silver rested before its communication with the metallic disks. This distance increases for every pair of disks added; and as the ball remains fixed at the point to which the screw has carried it, the motion of the instrument may be observed with great accuracy.

Its extreme sensibility.

This instrument is so sensible, that, if a slender glass tube be rubbed but twice, and brought near the apparatus, though several inches from its summit, it passes through the whole extent of its scale. It is for this reason the inventor calls it a *micro-electrometer*, because we can measure only very weak degrees of electricity with it.

## XXIV.

*Action of Phosphorus on the Solutions of Metals.* By Mr. SCHNAUBERT\*.

Phosphorus observed to precipitate metals, by Sage, Ilsemann, and Mrs. Fulhame.

SAGE† had already observed, that phosphorus precipitates the sulphates of copper and of manganese. After him Ilsemann‡ obtained a crystallization of silver in the humid way by means of phosphorus. Still more recently Mrs. Ful-

\* Van Mons's *Journal de Chimie*, Vol. VI. p. 95. Abridged from Goettling's *Chemisches Taschenbuch*.

† *Analyse chimique et Concordance des trois Régnes*.

‡ Crell's *Chemische Annalen*, 1789, Tom. II. p. 323.

hame\* published experiments on the reduction of some metals by phosphorus dissolved in ether. Lastly, Mr. Schnaubert Schnaubert's experiments. has undertaken a new investigation of the subject, the principal results of which are as follows:

#### *Gold.*

Two little bits of phosphorus were put into a nitro-muriatic Gold completely solution of gold diluted with a small portion of water. At the expiration of twenty-four hours the solution was completely precipitated in a metallic form by phosphorus. colourless, and pellicles of the colour of metallic gold swam on the surface of the liquid. The phosphorus itself was covered with a deep brown coating, and in this was observable in several places thin layers of reduced gold. At the place where the phosphorus was a black circle was perceived. The solution thus treated by phosphorus, had not a single atom of gold left in it.

#### *Silver.*

Some phosphorus, which was left for twenty-four hours in Silver precipitated in dendritic crystals, a nitric solution of silver diluted by water, was completely covered with metallic silver in the form of dendrites, the ramifications of which were directed upwards. During ebullition this remarkable crystallization of silver, which made the phosphorus appear as if garnished with points, assumed first a white colour, and afterward formed a light black mass, which at length became of a light brown colour. which boiling converted into a phosphure.

#### *Quicksilver.*

Mercury dissolved in nitric acid is precipitated on the phosphorus in the form of little metallic globules, which cover it entirely. By heating to ebullition the mercurial globules disappear, and a black mass without any metallic lustre is formed. Mercury precipitated in globules, which heat converts to a phosphure.

#### *Lead.*

At the ordinary temperature phosphorus did not act on the nitric solution of lead, though the digestion was continued for several days: at a boiling heat however a change was observed in the phosphorus, which was covered with a grey colour slightly metallic. Lead not reduced without a boiling heat,

\* Essay on Combustion, &c.

*Copper.*

*Copper.*

Copper power-  
fully acted on.

No metallic solution was more strongly attacked by phosphorus than that of copper in nitric acid. The phosphorus was no sooner introduced into the solution of copper, than it assumed a black colour. In twenty-four hours it was covered with a stratum of metallic copper in very thin layers \*; and the solution had become much paler. The application of heat caused drops of phosphorus to flow out upon the reduced copper, where they immediately assumed a black colour. These drops after a time were in their turn covered with a metallic coat of copper. After this the solution was perfectly colourless, and ammonia did not detect in it the least particle of metal.

Completely pre-  
cipitated from  
the solution.

*Tin.*

Tin partly re-  
duced, partly  
converted into  
phosphure.

Several bits of phosphorus were put into a solution of tin in nitro muriatic acid. The next day the phosphorus was coloured of a deep brown, only in some parts a metallic colour was observable. These metallic spots disappeared on boiling, and the phosphorus became still deeper coloured.

Sulphate of cop-  
per forms a beau-  
tiful experiment.

The phosphorus  
enveloped in a  
case of particu-  
larly malleable  
copper impervi-  
ous to air.

\* In making this experiment with a solution of sulphate of copper, and slightly heating the mixture, at first a vapour arises, consisting of phosphorus gas, that carries off with it some small particles of phosphorus, which take fire on the surface of the solution: but the extrication of this vapour soon ceases, and the phosphorus becomes hermetically enclosed in a box of copper, in which it is defended against any farther action of the sulphate, and even of the air, to whatever temperature short of fusing the copper it be afterwards exposed. The plate of copper that forms this covering is two or three lines thick: it possesses more tenacity than common copper, for it may be flattened with a hammer in different directions without cracking, which at the same time proves the great compressibility of the phosphorus; and it shines with a very pure metallic lustre. On opening the box carefully with a cutting instrument, the phosphorus is found in it retaining perfectly its form, filling its copper case completely, and not appearing even to have acted upon the sulphate.

Other metals did  
not produce this  
effect.

I did not obtain the same effect with several other metals which I tried, no doubt on account of their containing more oxygen.

VAN MONS.

*Manganese.*



*Manganese.*

Some bits of phosphorus were put into a sulphuric solution of manganese. The next day the phosphorus was of a deep brown colour, and on its surface was perceivable a pleasing mixture of colours, owing to the reduced metal. The mixture was then heated to ebullition, and after it had grown cold, the manganese was found reduced in the form of little radiating lines on the fused phosphorus, intermixed with a few small globules of the white colour of tin. Manganese reduced by it.

In these experiments we may observe, that the phosphorus, beside deoxidating the metals, united with the metals when reduced to form phosphures, as was evident in the solutions of silver, mercury, and tin. General condensation.

## XXV.

*Account of a new Pyrometer, which is capable of indicating Degrees of Heat of a Furnace. By Mr. J. G. SCHMIDT, of Yassy, in Moldavia. From the Author.*

WITHOUT entering into any detail concerning the substances best calculated for pyrometrical enquiries, I flatter myself that it will be admitted that those must receive the preference which are capable of regularly contracting or expanding, without altering their chemical properties, when subjected to elevated temperatures. Pyrometrical substances.

The permanently elastic aeriform fluids appear to me to be superior in those respects to any other bodies. Gases are the best for strong heat.

Let atmospheric air be freed from moisture by caustic alkalis, or other bodies, and included in a vessel of platina. This vessel A (*Fig. 3, Pl. VI.*), which may be made of any convenient size, is connected with the tube B B, of as fine a bore as possible. This tube is also made of platina, and reaches into a vessel C, which is filled with water up to *cc*, and into this the tube is fixed air-tight. Out of the vessel C rises a glass cylinder G hermetically sealed, including a thermometer, and a graduated tube F F is secured into the vessel C in a similar manner. Atmospheric air in a vessel of platina.

This

Use and application.

This is the whole construction of my pyrometer. To make use of it nothing more is necessary than to introduce the platina vessel A into the furnace the temperature of which is to be learned. The moment the included air is acted upon by the heat it expands, and expels the water up into the graduated tube FF. This rise will take place accordingly as heat increases. If care be taken that the air be cooled in the vessel C as much as possible (which will be the case from the large surface of water to which it is exposed), it is obvious that a volume of water equal to the volume of air in the vessels of platina, can never pass up into the tube. The refrigeration may be facilitated by the application of vaporizable fluids, such as ether, alcohol, &c.

If the degree of temperature be obtained which the air had before it was subjected to the experiment, and a proper allowance be made for the pressure of the water in FF, the true expansion of the air may thus be found, and compared with the respective temperatures.

## XXVI.

*New, easy, and economical Method of separating Copper from Silver. By Mr. GOETTLING\*.*

Sulphuric acid used instead of nitric on account of cheapness,

and with perfect success.

Description of the process.

THERE are four methods of separating copper from silver, all of which require the alloy to be dissolved in nitric acid. As this acid is very dear, Mr. Goettling thought of using the sulphuric in its stead, which is comparatively very cheap. His success perfectly equalled his expectation, and the following is his method :-

Having ascertained by the touchstone, or in any other way, the proportion of silver contained in the alloy, take one part of sulphuric acid for every part of silver, and for every part of copper three parts and three-fifths of a part of the same acid. Dilute the acid with half its weight of water, and pour it into a matrafs on the alloy reduced to very small pieces. In order

\* Translated from Van Mons's *Journal de Chimie*, Vol. VI. p. 77. Originally published in the *Taschen-Buch fuer Scheidekuenstler*, and abridged by Van Mons.

to promote the action of the acid, it is of use to put one part more to every sixteen parts of the alloy. The matrafs is then to be placed in a sand-heat, and the acid brought to a state of ebullition. In two or three hours time the alloy is commonly dissolved and converted into sulphate, particularly if care be taken to stir the mass from time to time with a glass spatula. This mass is thick, and frequently hard. While it is still hot, six or eight times its weight of boiling water is to be added to it, and it is to be left some time longer on the fire. The sulphate of copper will be dissolved, and great part of the sulphate of silver will be precipitated. The operator will now examine whether the whole be completely dissolved; and if it be, a plate of copper, or some pieces of copper or halfpence tied up loosely in a piece of coarse linen, must be suspended in the mixture, and the whole kept boiling for some hours. The sulphate of silver will thus be decomposed, and the silver separated in the metallic state.

To ascertain whether the separation be complete, a few drops of solution of muriate of soda are to be dropped into a little of the liquor. If a cheeselike precipitate be formed, it is a proof, that all the silver is not separated, and in this case the ebullition with the copper must be continued longer. After the whole of the silver is separated, the liquor is to be poured off, the precipitated silver is to be well washed, and the entire separation of the cuprous salt is to be ascertained by the addition of a few drops of liquid ammonia to the water with which the precipitate has been washed, which, if it contain any copper, will be rendered blue by the ammonia. After the silver is thoroughly freed from the sulphate of copper, it may be kept in the state of powder as it is, or it may be fused with a fourth part at most half its weight of sulphate of potash.

Mode of ascertaining whether the separation be complete.

May be kept in powder, or fused with sulphate of potash.

The water poured off is then to be mixed with what was used for washing the precipitate, and evaporated in a copper pan, so as to obtain the sulphate of copper by crystallization. The blue vitriol thus produced will be at least equal in value to the sulphuric acid employed.

Blue vitriol obtained equal to the cost of the acid.

If any parts of the alloy remained undissolved, it should be separated by decantation, and reserved for a future operation.

Undissolved alloy to be set by.



## SCIENTIFIC NEWS.

*Extract of a Letter from BRUGNATELLI, concerning the non-existence of the charged Pile\*.*

VOLTA has made many experiments on piles composed of a single metal, and a single wet stratum, which, from being inactive by themselves, become more or less active after affording a passage for a longer or shorter space of time to an electric current set in motion by active pile, &c.

Ritter asserted that a pile composed of one fluid, and one metal, was capable of being charged by another pile, but the fact is, that the fluid is converted into two different fluids by the electric current.

Ritter, the most judicious of the galvanic philosophers of Germany, has asserted, as Volta says, that the active pile, or common electrometer, transmits a real charge to the pile that is itself inactive, which it therefore calls the *charged pile*. Volta however has convinced himself, that no charge is transmitted, but, by virtue of the ordinary chemical action, the electric current being continued, changes the single wet stratum interposed between two pieces of gold, for example, into two different fluids, one acid, by which the electric current issues out of the metal, and the other alkaline, by which it enters; which constitutes a pile of the second order, namely, of one metal and two fluids of different natures, the action of which however does not continue long, because the fluids soon mix.

Mechanical  
work by Mr.  
Gregory.

Mr. GREGORY of the Royal Military Academy, Woolwich, has now in the press a Treatise on Mechanics, which is intended to be published in two volumes octavo. The first will be devoted chiefly to the theory, and will be divided into five books under the several heads of statics, dynamics, hydrostatics, hydrodynamics, and pneumatics. The second volume will be chiefly appropriated to practical and descriptive subjects, and will commence with general remarks, rules, and tables, relative to the nature, construction, and simplification of machinery; the effects of friction, and the rigidity of cords; and estimates of the varied energy of different first movers, &c. These will be followed by descriptions arranged alphabetically, of about 100 of the most curious, useful, and important machines. In this latter part, Mr. Gregory has been promised communications from some celebrated civil engineers, so that he hopes, on the whole, to render the work in some measure deserving the attention of those who are engaged in the cultivation and improvement either of the theory or the practice of mechanics.

\* Van Mons *Journal de Chimie*, Vol. VI. p. 132.



Fig. 1.

*Captain Borden's Method of casting Iron Bars.*

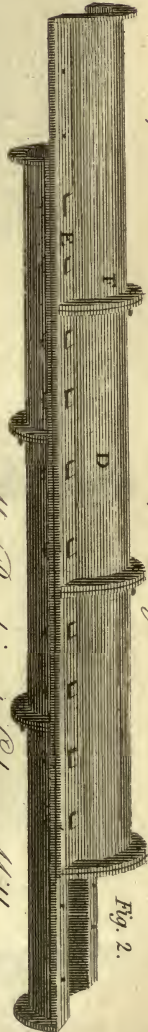


Fig. 2.

*Mr. Sanderson's Colour Mill.*

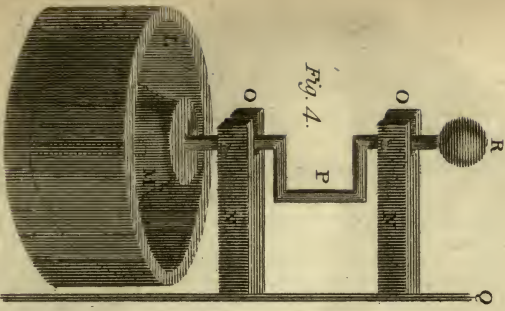


Fig. 4.

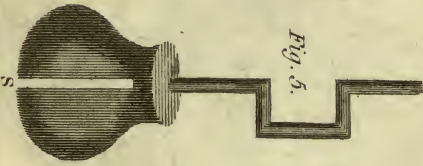


Fig. 5.

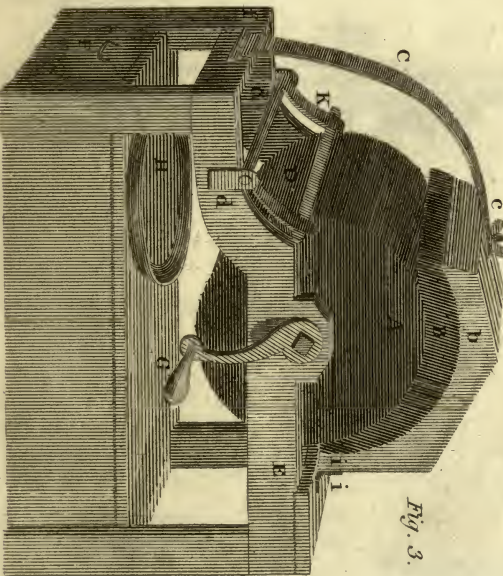


Fig. 3.

*Improved Antigo Mill.*

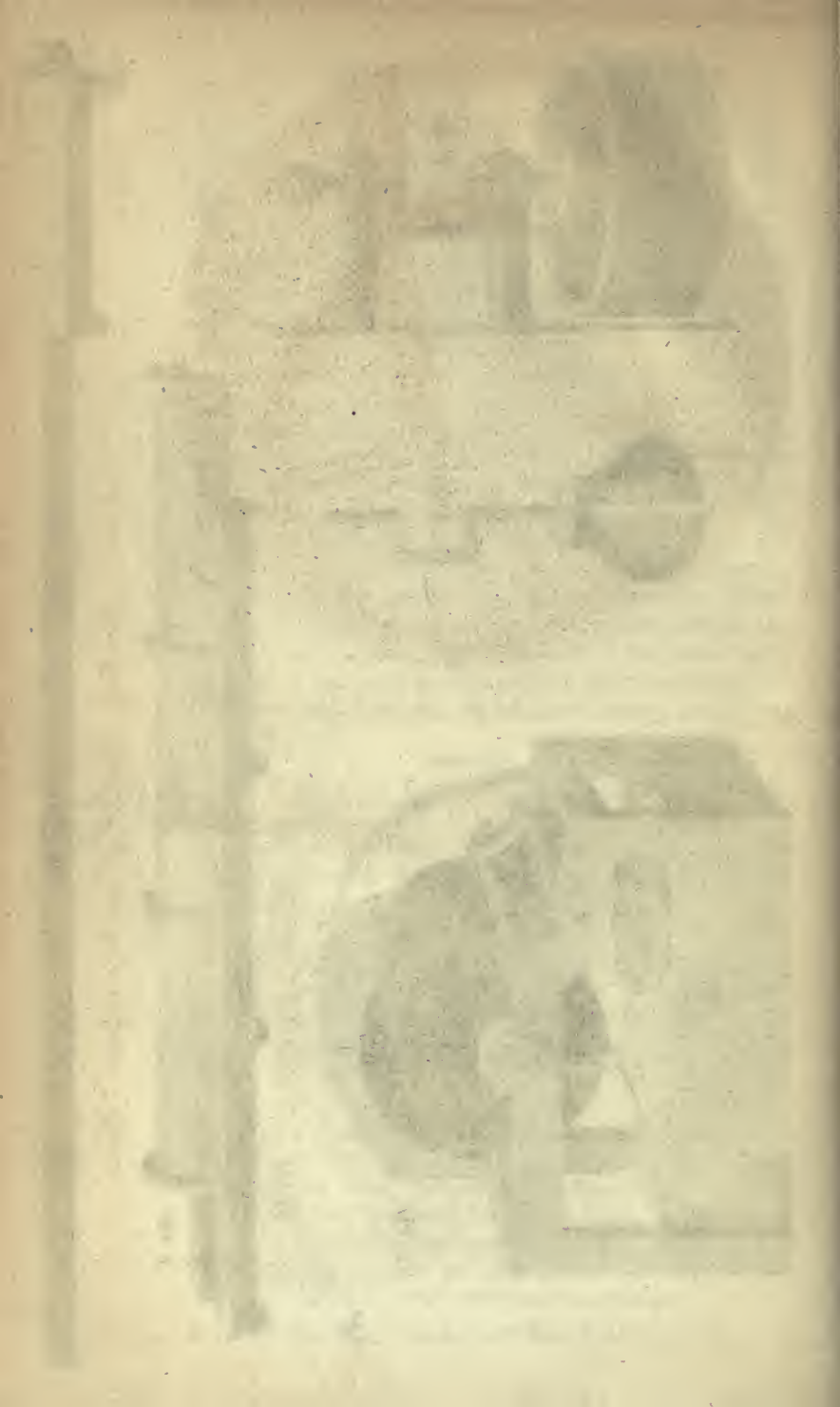
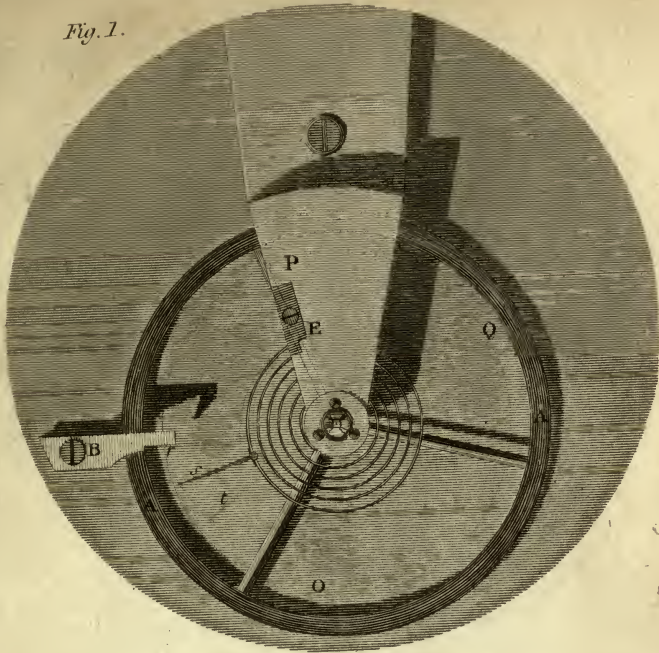
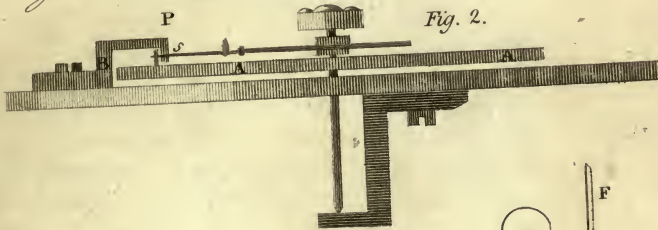




Fig. 1.

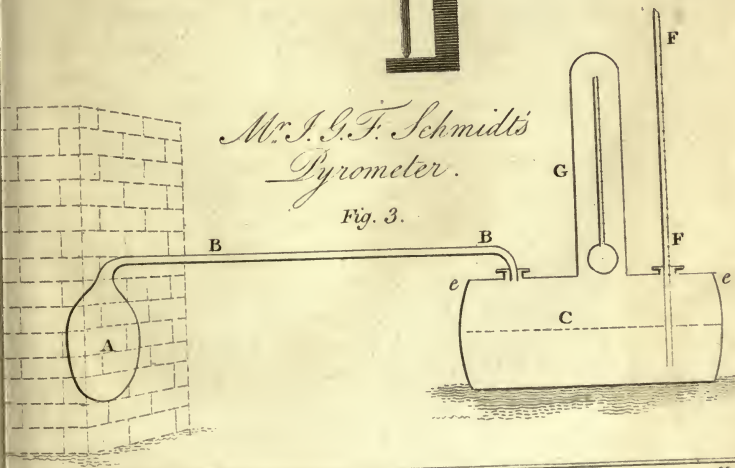


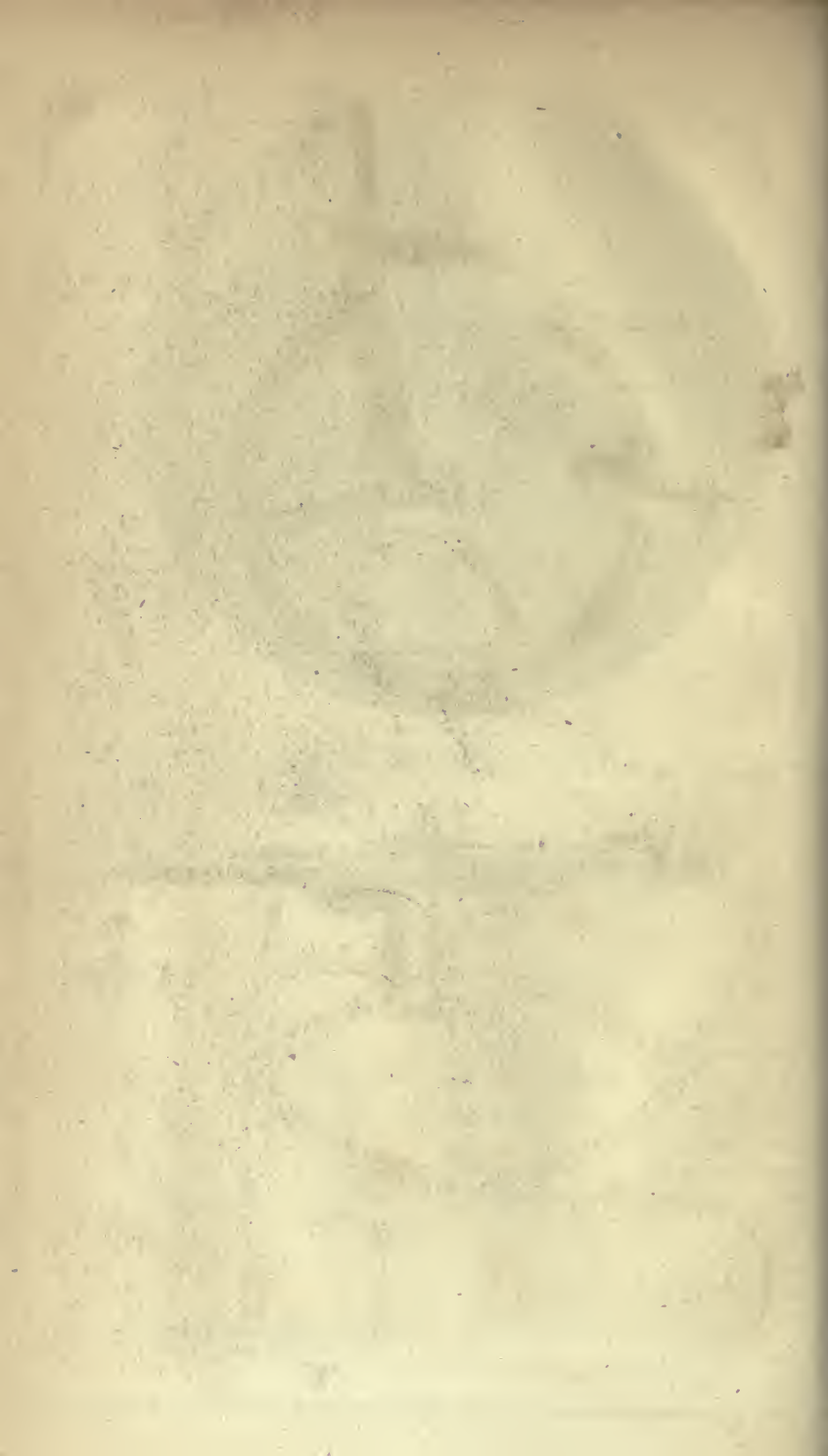
*Mr. Hardy's method of banking the balance of a Time keeper.*



*Mr. J. G. F. Schmidt's  
Pyrometer.*

Fig. 3.





# A JOURNAL

OF

NATURAL PHILOSOPHY, CHEMISTRY,

AND

THE ARTS.

JULY, 1805.

## ARTICLE I.

*Remarks on the Estimation of the Strength of Horses. In a Letter from Mr. O. GREGORY, of the Royal Military Academy, Woolwich.*

To Mr. NICHOLSON.

SIR,

THE remarks of your ingenious correspondent, Mr. Hornblower, on the various estimates of the *Power of a Horse*, and the absurdity of adopting a quantity so fluctuating and so difficult to ascertain, as a common measure by which the powers and effects of steam engines and other machines are to be estimated and compared, have induced me to throw together a few observations on the same subjects; the theoretic part of which, though familiar to most men of science, seems not to be always known, or at least recollected, by some persons who are employed in the practice; and which are altogether much at your service for insertion in the Journal, if you think them likely to be of any utility.

Dr. Desaguliers has given another estimate of the labour of a horse, beside that mentioned by Mr. Hornblower, and which indeed does not seem very consistent with it; for in vol. II. p. 251. of his *Experimental Philosophy*, he affirms that a horse in

Estimate of horse power by Desaguliers. 200lb. eight hours per day, at  $2\frac{1}{2}$  miles per hour; or  $3\frac{2}{3}$  feet per second,

VOL. XI.—JULY, 1805.

L



—by Sauveur  
189lb. avoird.  
three feet per  
second,

—by the author  
130lb. three feet  
per second.

Misapplication  
of the general  
principle that  
gain in power is  
lost in time.

an advantageous situation, is able to draw 200lbs. eight hours a day, walking at the rate of  $2\frac{1}{2}$  miles in an hour, or  $3\frac{2}{3}$  feet in a second. This statement of the power of a horse, though it is not so great as that which is arbitrarily assumed by Messrs. Boulton and Watt, exceeds the determination of M. Sauveur, who estimates the mean effort of a horse at 175 French, or 189 avoirdupois lbs. with a velocity of rather more than three feet per second; and it probably exceeds Mr. Smeaton's statement of 550lbs. moved 40 feet in a minute; though, as will soon be seen, we are not furnished with proper data to institute a comparison between these various results. It is probable, however, as observed by the ingenious contributor of the article at page 216, vol. IX. of your Journal, that "the lowest of these performances is more than equal to the average power of a horse employed in husbandry for eight hours per day." So far as my own observations on this point extend, I am inclined to conclude that the average work of a stout London cart horse, for eight hours in a day, is little if any more than 130lbs. moved at the rate of three feet in a second, or  $2\frac{1}{2}$  miles per hour. But this it would be ridiculous to assume positively as a universal unit of measure, in a case where the causes of variety are so numerous, and my opportunities of experiment comparatively few. The estimate just given, it should be observed, is not intended to express what a horse can draw upon a wheel carriage, where friction alone is to be overcome, after the load is once put into motion, and where a horse will often draw much more than 1000lbs. but the weight which a horse would raise out of a well, &c. the animal acting by a horizontal line of traction turned into the vertical direction by a simple pulley or roller, whose friction is reduced as much as possible.\*

Before we can institute any comparison between the results of different experiments, it will always be necessary to enquire what machine was interposed between the weight moved and the animal, in each case, that we may thence deduce the real velocity with which the animal moved, from the velocity of the weight or load given by the observations. This is too fre-

\* The late Mr. More, Sec. to the Society of Arts, found by the interposition of a graduated spring instrument between the horse and his work, that the re-action was between 70 and 80lb. when the velocity was three miles in an hour. I think the work was ploughing. See *Philos. Journal*, quarto, Vol. III. 136. N.

quently

quently omitted in consequence of an implicit reliance upon a maxim, which, though highly useful under proper restrictions, is far from universal in its application. In the case before us, if we admit the maxim now alluded to, namely, that *what is gained in power is lost in time*, with regard to the machine through whose intervention the velocity of the weight is rendered different from that of the horse, it would be unsafe to adopt it in the appreciation of the varied energy of the animal when moving with different velocities. The reason of this is obvious. The energy of the horse is obliged to be employed not only in overcoming the weight or resistance which opposes his progress, but in part in moving *himself*; for the particles which constitute his frame possess weight and inertia, and therefore cannot be put into motion without effort. Hence it follows that there is a certain velocity, which may be denoted by  $U$ , with which, when the animal moves, his whole power will be employed in producing his own motion solely, without being able to move any other body. If a body whose mass is  $M$  be attached to the horse, so that he cannot move without giving an equal velocity to the extraneous body, the same effort being employed both in moving the animal and the mass attached; the velocity  $V$ , with which they move must necessarily be less. And if  $M$  be farther increased while the weight and energy of the horse continue the same, the velocity  $V$  will be still farther diminished; and thus as  $M$  increases  $V$  will diminish, until when  $M$  arrives at a certain magnitude,  $W$ , the animal is unable to make any progressive motion, and exerts his force at what is called a dead pull. If  $M$  exceeds  $W$ , then will  $V$  become negative, and instead of the animal advancing with the load, the load will compel him to move backwards, and no useful work can be accomplished.

An horse, having himself to carry, does not exert his force against his work only.

Now these circumstances may be expressed algebraically, by the general formula  $M \propto (U - V)^n$ , in which the exponent  $n$  can only be determined by means of judicious and numerous experiments, where the magnitude of  $M$  should be ascertained for many variable values of  $V$  between the terms  $V = U$ , and  $V = 0$ . From this theorem, following the common rules for the maxima and minima of quantities, it may readily be found that in order to have the *useful* work done the greatest possible, we must increase or decrease the weight till  $V$  becomes  $= \frac{U}{n+1}$ ,

Method of computing the effect of horses.



when the performance will be denoted by  $\frac{n^2}{n+1} \times W U$   
 $(n+1)$

And if the value of  $V$  thus exhibited be once ascertained experimentally, we need never be apprehensive of a material loss by a small variation from it; for by a well known property of those quantities which admit of a proper maximum and minimum, a value assumed at a moderate distance from either of these extremes will produce no sensible change in the effect.

**Example.**

In some of the actions of men, such as dragging a boat along a canal, &c. the value of  $n$  in the preceding theorems has been found to be nearly  $= 2$ . And the draught of horses is conformable to a law not widely different. The best experiments which have yet been made on this point with regard to horses drawing in nearly rectilinear paths, lead us to conclude that  $n$  is then very nearly  $= \frac{9}{4}$ , in the expression  $M \propto (U - V)^{\frac{n}{2}}$ . Assuming therefore for the utmost walking velocity of a horse, the value  $U = 9$  feet per second, a value which is quite high enough, any proposed estimates of the strength of this animal may be compared with facility. Thus, for example, let us enquire which is greater, the estimate of Mr. More (mentioned by Mr. Hornblower) of 80lbs. three miles per hour, or  $4\frac{2}{3}$  feet per second; or that of 130lbs. moved at the rate of three feet per second? Here we shall have

$$(9-3)^{\frac{9}{4}} : (9-4\frac{2}{3})^{\frac{9}{4}} :: 130 : 71\frac{1}{2}\text{lbs. nearly.}$$

The operation may easily be performed by means of a table of logarithms, and shews that the mean estimate I have laid down, when reduced to the same velocity as that by Mr. Moore, furnishes a result less than his by  $8\frac{1}{2}$ lbs. Which of these is the most accurate can only be determined by future experiments.

On the power  
of horses walk-  
ing in circular  
paths;

If, however, either of these estimates should be adopted, it may be proper to remark that they would not hold with regard to the power of horses working in circular paths; yet, if it be at all proper to use *horse powers* in estimating the energy of machines, it seems most natural to take these powers as exerted by the animal in a round walk; so that it is still necessary to have a series of experiments to determine the values of  $n$ , and the relation of  $M$  and  $V$  when horses draw in circular walks of different radii. I say, of *different* radii, because it is certain that *ceteris paribus*, the greater the radius of the circle

—of different  
radii.

in



in whose circumference the animal moves, the less fatigue will be occasioned by that kind of motion. Indeed it is obvious, that since a rectilinear motion is the most easy and natural for the horse, the less the line in which he moves is curved, with the greater facility he will walk over it, and the less he need recline from the vertical position. Besides this, with equal velocity at the circumference, the centrifugal force will be less in the greater circle, which will proportionally diminish the friction of the cylindrical part of the trunnions, and the labour of moving the machine. And farther, the greater the radius of the horse-walk, the nearer the chord of the circle in which the horse draws is to coincidence with the tangent, which is the most advantageous position of the line of traction. Hence it follows, that although a horse *may* draw in a walk of 18 feet diameter, yet he will work with far greater ease in one whose diameter is 35 or 40 feet; and it is very desirable that an experimental enquiry should be made to ascertain the proportion and absolute quantity of work in different circles.

The larger walks are most advantageous.

I am of opinion that it would not be difficult to make some useful experiments, while work was actually carrying on at any horse mill, or machine where horses are constrained to move in a circular walk. The simple drawing which accompanies this letter will assist in conveying a clear idea of the method which I fancy might be advantageously adopted. Let *AB Fig. 1. Plate X.* be the vertical shaft to which the horizontal horse poles *AC, AD*, are attached. Let one horse work the machine by drawing at the ear *E*; but, instead of the transverse bar to which the harness is fixed being simply hung upon the hook *h*, let a good spring steelyard be interposed between that cross-bar and the hook, the graduations of which shall, when the machinery is put into motion, indicate the resistance (in lbs.) overcome by the animal, including the weight of the mass moved, the friction, &c. Near the extremity of the opposite horse-pole *AD*, let there be fixed a strong and correct common steelyard, whose divisions shall shew the various weights from 40 or 50 to 200 lbs. and whose centre of motion shall be at the point *f* on the fixed stand. Let the cord *c* which is fastened to the shorter arm of this steelyard, pass (with as little friction as possible) over the pulley *p*, and thus, being turned into the horizontal direction, or rather, inclining a little upwards, let it be fixed to the cross bar

Description of a mechanical apparatus for measuring the re-action in mills.

Description of a  
mechanical ap-  
paratus for  
measuring the  
re-action in mills.

bar of the harness of a second horse, equal in point of strength to the former. Then if the two horses, thus attached to the ears E and F, be made to pass over the walk in the same direction, following each other constantly at the distance of a semi-circumference; while that which draws at the ear E overcomes the whole pressure and resistance opposed by the work, the other which draws at F by the cord over the pulley *p*, will raise the weight *w* of the steelyard; which therefore, by being moved to and fro upon the arm *fi*, may be brought to exhibit an exact counterpoise, or measure of the exertion and power of the horse. And in order to ensure the greatest degree of accuracy in this respect, the motion of the two animals, and the position of the weight *w*, should be so adjusted, that the same weight should be shewn by the graduations both of the spring and of the lever steelyard. The shaking of the machinery will in some measure disturb the effect; but an ingenious manager of the experiments will find means of checking this: and as to the centrifugal force to which the weight *w* is exposed, it will never be of any material consequence in any of the slow motions which will be produced by this kind of work.

Each experiment should occupy the space of a fair day's work for the horses; for the conclusions deduced from shorter and irregular efforts are always erroneous in excess, and should be guarded against. The rate at which the animals move may readily be ascertained from the known circumference of the walk, and the number of rounds they are observed to make in 10 or 15 minutes. Thus, by continuing the experiments day after day, varying the velocity of the motion in some cases, and the radius of walk in others, such a series of results might at length be obtained, as would in a great measure remove the obscurity and doubt in which this business is at present enveloped. It is scarcely necessary to suggest the propriety of making a few experiments with a view of determining how far a load upon the back of each draught horse, would assist him in his labour. Nor can it be requisite to point out in what way, by means of such steelyards properly applied to waggons, &c. upon tolerably smooth roads, and two horses marching abreast, (one drawing the load, the other raising the weight,) experiments might be instituted to ascertain the magnitude of the efforts of horses when drawing



in rectilinear paths. Judicious experiments having these purposes in view, would certainly be beneficial, as they would enable us to tell what advantages might be expected from the labours of this useful quadruped in different circumstances. But with respect to the adoption of "*horse power*" as a unit of force in estimating the power of steam engines, &c. I confess that if it were as well known, and as un-  
The adoption of an arbitrary unit from the power of an horse.

Before I close this letter, already perhaps too long, I beg permission to say a few words respecting the measure which is generally employed to determine the mechanical effect produced. This is the measure of the deservedly celebrated Mr. John Smeaton, who says that "the weight of a body multiplied by the height through which it descends, while driving a machine, is the only proper measure of the power expended; and that the weight multiplied by the height through which it is uniformly raised is the only proper measure of the effect produced." Mr. Smeaton was led to the use of this measure by his professional habits; and many who in this respect pay too great a deference to his authority, have adopted this measure as universal and preferable to any other. Taking this as a popular measure easy to recollect, and simple in its application, it undoubtedly has its uses; but in many instances it is inadequate to the purpose for which it is proposed. Smeaton's measure of mechanical power and effect. He says it is as the product of the weight into the height passed through in either case;

The late Professor Robison has some excellent observations on this subject, in the article *Machinery*, *Sup. Encyclopædia Britan.* where he lays down the just measure to which the scientific investigator will generally have recourse. "We take, says he, for the measure, (as it is the effect) of exerted mechanical power, the quantity of motion which it produces (or whose accumulation it prevents) by its uniform exertion during some given time. We say *uniform exertion*, not because this uniformity is necessary, but only because, if any variation of the exertion has taken place, it must be known in order to judge of the power."

rectified by  
 Professor  
 Robison.

A single



Smeaton's measure is not applicable to an horse sustaining (but not raising) a weight.

A single instance may be adduced, to which the measure of Mr. Smeaton is inapplicable, and in which we must have recourse to some such measure as that mentioned by Professor Robison. Suppose that a horse while standing still sustains by means of a rope and simple fixed pulley, a mass of a hundred-weight, and thus keeps it suspended at the top of a well, for the space of a minute. Neither the animal nor the weight moves, but shall we therefore say, in conformity, as it would seem, with Mr. Smeaton's measure, that there is no power expended, and no effect produced? On the contrary we know there is a power expended, and that the effort if sufficiently long continued would completely tire the horse. The effect which is produced is the annihilation of the simultaneous action of gravity upon the suspended mass; consequently, the effect produced is equal, and contrary to the momentum that would be generated by gravity in the space of a minute. So that  $50 \times 32 \frac{1}{2} \times 112 = 216160$ , is the proper representative of the power expended, as well as of the work done. Were the rope to be cut and the weight suffered to fall for a minute, the same number would likewise denote the labour of the horse in restoring it to its original place, provided that could be accomplished in an equal space of time, without the horse changing his situation.

General statement.

It may not, perhaps, be entirely useless to state this matter rather more universally. To this end, let  $M$  represent any mass or body,  $g = 32 \frac{1}{2}$  feet, the velocity communicated to a body falling freely in the first second of time, and  $t$  an indefinitely small portion of any time whatever  $t$ . Then will  $g t$  be the velocity generated in the instant  $t$ , and  $M g t$  the corresponding quantity of motion; this, therefore, measures the effort which must be exerted at each instant to sustain the weight, whether that effort be applied immediately, or through the intervention of a single fixed pulley. Hence it follows, that during the whole time  $t$ , the force will have consumed a quantity of motion equal to  $\int M g t = M g t$ : that is to say, if  $t$  denote the time at the end of which the agent is no longer able to sustain the mass  $M$ , we may regard  $M g t$  as being an adequate measure of the force  $\phi$  of that agent. If the agent not only prevent the mass from falling, but actually raise it with a given uniform velocity  $V$  during the whole time  $t$ , then we must add the quantity of motion  $MV$  to the former, which

which gives  $\phi = M V \times M g t = M (V \times g t)$ . And lastly, if the agent possess inertia, its mass must also be considered. Thus, in the case of a horse whose mass is  $H$ , moving along with the velocity  $V$  during the time  $t$ , and raising the mass  $M$ , we shall have  $\phi = (M \times H) V \times M g t$ . And from similar principles formulæ may be investigated to represent the power of a first mover in more complicated cases.

It will after all, be proper to distinguish carefully between the quantity of power expended, and that portion of it which is usefully employed: but a due consideration of this would require to be distinguished. too widely extend the limits of the present communication. Indeed I ought to apologize to yourself, and the scientific part of your readers, for dwelling so long as I have done upon topics which are well known to all who are conversant in the theory of mechanics: but if those, for whose use this letter is chiefly intended, shall derive some precise information, or add to the stock of their practical knowledge, by any hints of mine, I shall not fear being heavily censured for having entered thus into minutiae.

I am, Sir,

Your's very respectfully,

OLINTHUS GREGORY.

Royal Mil. Academy, Woolwich.

June 10th, 1805.

## II.

*An Account of some Analytical Experiments on a Mineral Production from Devonshire consisting principally of Alumine and Water. By HUMPHRY DAVY, Esq. F. R. S. Professor of Chemistry in the Royal Institution. From the Philosophical Transactions in 1805.*

### I. Preliminary Observations.

THIS fossil was found many years ago by Dr. Wavell, in a History of a quarry near Barnstaple: Mr. Hatchett, who visited the place in 1796, described it as filling some of the cavities and veins in a rock of soft argillaceous schist. When first made known, it

it was considered as a zeolite; Mr. Hatchett, however, concluded, from its geological position, that it most probably did not belong to that class of stones; and Dr. Babington, from its physical characters, and from some experiments on its solutions in acids, made at his request by Mr. Stocker, ascertained that it was a mineral body, as yet not described; and that it contained a considerable proportion of aluminous earth.

It is to Dr. Babington that I am obliged for the opportunity of making a general investigation of its chemical nature; and that gentleman liberally supplied me with specimens for analysis.

## II. *Sensible Characters of the Fossil.*

**Its sensible characters.**  
Radiated hemispherical groups; white, silky, hard, little tenacious.

The most common appearance of the fossil is in small hemispherical groups of crystals, composed of a number of filaments radiating from a common center, and inserted on the surface of the shist; but in some instances it exists as a collection of irregularly disposed prisms forming small veins in the stone: as yet, I believe, no insulated or distinct crystal has been found. Its colour is white, in a few cases with a tinge of gray or of green, and in some pieces (apparently beginning to decompose) of yellow. Its lustre is silky; some of the specimens possess semi-transparency, but in general it is nearly opaque. Its texture is loose, but its small fragments possess great hardness, so as to scratch agate.

**Other characters.**

It produces no effect on the smell when breathed upon, has no taste, does not become electrical or phosphorescent by heat or friction, and does not adhere to the tongue till after it has been strongly ignited. It does not decrepitate before the flame of the blow-pipe; but it loses its hardness, and becomes opaque. In consequence of the minuteness of the portions which it is found, few of them exceeding the size of a pea, it is very difficult to ascertain its specific gravity with any precision; but from several trials I am disposed to believe, that it does not exceed 2,70, that of water being considered as 1,00.

## III. *Chemical Characters of the Fossil.*

**Chemical habits.**  
Soluble in acids and in f. alkalis.

The perfectly white and semi-transparent specimens of the fossil are soluble both in the mineral acids and in fixed alkaline lixivia by heat, without sensibly effervescing and without leaving



leaving any notable residuum; but a small part remains undissolved, when coloured or opaque specimens are exposed to the alkaline lixivium.

A small semi-transparent piece, acted on by the highest Forge heat gave opacity, but did not fuse it. Loss one-fourth.

After the experiment it adhered strongly to the tongue, and was found to have lost more than a fourth of its weight. Water and alcohol, whether hot or cold, had no effect on the fossil. When it was acted on by a heat of from  $212^{\circ}$  to  $600^{\circ}$  Fahrenheit in a glass tube, it gave out an elastic vapour, which when condensed appeared as a clear fluid possessing a slight empyreumatic smell, but no taste different from that of pure water. It emitted water.

The solution of the fossil in sulphuric acid, when evaporated sufficiently, deposited crystals which appeared in thin plates, and had all the properties of sulphate of alumine; and the solid matter, when redissolved and mixed with a little carbonate of potash, slowly deposited octahedral crystals of alum. The solid matter precipitated from the solution of the white and semi-transparent fossil in muriatic acid, was in no manner acted upon by solution of carbonate of ammonia, and therefore it could not contain any glucine or ittria; and its perfect solubility without residuum in alkaline lixivium shewed that it was alumine. Sulphuric solution afforded sulphate of alumine. Muriatic solution contained alumine only.

When the opaque *varieties* of the fossil were fully exposed to the agency of alkaline lixivium, the residuum never amounted to more than one-twentieth part of the weight of the *whole*. In the white opaque variety, it was merely calcareous earth, for when dissolved in muriatic acid, not in excess, it gave a white precipitate when mixed with solution of oxalate of ammonia, and did not affect solution of prussiate of potash and iron. The white varieties contained some lime:

In the green opaque variety, calcareous earth was indicated by solution of oxalate of ammonia: and it contained oxide of manganese; for it was not precipitated by solution of ammonia; but was rendered turbid, and of a gray colour, by solution of prussiate of potash and iron. and the green, also manganese:

The residuum of the alkaline solution of the yellow variety, when dissolved in muriatic acid, produced a small quantity of white solid matter when mixed with the solution of the oxalate of the yellow, a trace of iron.

of

of ammonia, and gave a light yellow precipitate by exposure to ammonia; but after this, when neutralized, it did not affect prussiate of potash and iron, so that its colouring matter, as there is every reason to believe, was *oxide of iron*.

#### IV. *Analysis of the Fossil.*

Analysis of 80 grains.

1. Exposure to heat.

Eighty grains of the fossil consisting of the whitest and most transparent parts that could be obtained, were introduced into a small glass tube having a bulb of sufficient capacity to receive them with great ease. To the end of this tube, a small glass globe attached to another tube, communicating with a pneumatic mercurial apparatus, was joined by fusion by means of the blow-pipe.

The bulb of the tube was exposed to the heat of an Argand lamp; and the globe was preserved cool by being placed in a vessel of cold water. In consequence of this arrangement, the fluid disengaged by the heat, became condensed, and no elastic matter could be lost. The process was continued for half an hour, when the glass tube was quite red.

Water came over.  
19 grains

A very minute portion only of permanently elastic fluid passed into the pneumatic apparatus, and when examined, it proved to be common air. The quantity of clear fluid collected, when poured into another vessel, weighed 19 grains, but when the interior of the apparatus had been carefully wiped and dried the whole loss indicated was 21 grains. The 19 grains of fluid had a faint smell, similar to that of burning peat; it was transparent, and tasted like distilled water: but it slightly reddened litmus paper. It produced no cloudiness in solutions of muriate of barytes, of acetite lead, of nitrate of silver, or of sulphate of iron.

Very slightly acid.

2. Solution of the residue in sulph. acid: precipitation and re-solution by alkali. One grain and a quarter of lime remained undissolved.

The 59 grains of solid matter were dissolved in diluted sulphuric acid, which left no residuum; and the solution was mixed with potash, in sufficient quantity to cause the alumine at first precipitated again to dissolve. What remained undissolved by potash, after being collected and properly washed, was heated strongly and weighed; its quantity was a grain and quarter. It was white, caustic to the taste, and had all the properties of lime.

3. Nitric acid was added in excess, and then carbonate of am-

The solution was mixed with nitric acid till it became sour. Solution of carbonate of ammonia was then poured into it till the effect of decomposition ceased. The whole thrown into a filtering



filtrating apparatus left solid matter, which when carefully washed and dried at the heat of ignition, weighed 56 grains. They were pure alumine: hence the general results of the experiments, when calculated upon, indicated for 100 parts of this specimen,

Of alumine	-	70	Component
Of lime	-	1.4	parts.
Of fluid	-	26.2	
Loss	-	2.4	

The loss I am inclined to attribute to some fluid remaining in the stone after the process of distillation; for I have found, from several experiments, that a red heat is not sufficient to expel all the matter capable of being volatilized, and that the full effect can only be produced by a strong white heat.

Fifty grains of a very transparent part of the fossil, by being exposed in a red-heat for fifteen minutes, lost 13 grains; but when they were heated to whiteness, the deficiency amounted to 15 grains, and the case was similar in other trials.

Different specimens of the fossil were examined with great care, for the purpose of ascertaining whether any minute portion of fixed alkali existed in them; but no indications of this substance could be observed; the processes were conducted by means of solution of the unaltered fossil in nitric acid; the earths and oxides were precipitated from the solution by being boiled with carbonate of ammonia; and after their separation, the fluid was evaporated to dryness, and the nitrate of ammonia decomposed by heat, when no residuum occurred.

A comparative analysis of 30 grains of a very pellucid specimen was made by solution in lixivium of potash. This specimen lost 8 grains by long-continued ignition, after which it easily dissolved in the lixivium by heat, leaving a residuum of a quarter of a grain only, which was red oxide of iron. The precipitate from the solution of potash, made by means of muriate of ammonia, weighed, when properly treated, 21 grains.

Several specimens were distilled in the manner above described, and in all cases the water collected had similar properties. The only test by which the presence of acid matter in it could be detected, was litmus paper; and in some cases the effect upon this substance was barely perceptible.



V. *General Observations.*

The acid matter  
in the water was  
not nitric, mur.  
or sulph.

I have made several experiments with the hope of ascertaining the nature of the acid matter in the water; but from the impossibility of procuring any considerable quantity of the fossil, they have been wholly unsuccessful. It is, however, evident, from the experiments already detailed, that it is not one of the known mineral acids.

It is foreign to  
the stone.

I am disposed to believe, from the minuteness of its proportion, and from the difference of this proportion in different cases, that it is not essential to the composition of the stone; and that, as well as the oxide of manganese, that of iron, and the lime it is only an accidental ingredient, and on this idea the pure matter of the fossil must be considered as a chemical combination of *about thirty parts of water and seventy of alumine.*

Alumine has an  
affinity for  
water.

The experiments of M. Theodore de Saussure on the precipitation\* of alumine from its solutions, have demonstrated the affinity of this body for water; but as yet I believe no aluminous stone, except that which I have just described, has been found, containing so large a proportion of water, as thirty parts in the hundred.

Diaspore exam.  
by Vauquelin,  
contains 80 alu-  
mine and 16  
water.

The diaspore, which has been examined by M. Vauquelin, and which loses sixteen or seventeen parts in the hundred by ignition, and which contains nearly eighty of alumine, and only three of oxide of iron, is supposed by that excellent chemist to be a compound of alumine and water. Its physical and chemical characters differ however very much from those of the new fossil, and other researches are wanting to ascertain whether the part of it volatilized by heat is of the same kind.

Cornish mineral  
resembling the  
subject of this  
paper.

I have examined a fossil from near St. Austle, in Cornwall, very similar to the fossil from Barnstaple in all its *general chemical characters*; and I have been informed, that an analysis of it, made by the Rev. William Gregor some months since, proves that it consists of similar ingredients.

Proposed names.

Dr. Babington has proposed to call the fossil from Devonshire *Wavellite*, from Dr. Wavell, the gentleman who discovered it; but if a name founded upon its chemical composition be preferred, it may be denominated *Hydrargillite*, from *ὕδωρ* water, and *ἀργίλλος* clay.

\* Journal de Physique, Tom. LII. p. 280.

## III.

*On the Aberrations of Light passing through Lenses. By Mr. EZEKIEL WALKER.*

THE discovery of the aberration of the rays of light, caused by their unequal refrangibility, formed a new area in the science of optics. It is the foundation of all *Sir Isaac Newton's* discoveries in light and colours; and also the foundation of most of the useful improvements in the construction of optical instruments, that have done so much honour to our country. And this science may still derive further improvements from the same discovery, not only in the construction of instruments, but also in explaining some curious phenomena in nature.

But before I attempt to show how the use of this property of vision may be extended, it seems necessary to give a short account of that kind of aberration which arises from the unequal refrangibility of the differently coloured rays of light; the other aberration, or that which is caused by the spherical figure of the lens, is not here considered as being inconsiderable when compared with the former.

Therefore let *A C B*, *Fig. 2, Plate X.* represent a plano-convex lens; *P A* and *R B* two pencils of white or compound rays of light, falling upon it at the points *A* and *B* in a direction parallel to its axis. Also let *A x v* and *B y v* be the red or least refrangible rays, and *A g y* and *B q x* the violet, or most refrangible. The red ray from *A* will cut the violet ray from *B* at the point *x*, and the red ray from *B* will cut the violet ray from *A* at the point *y*; through these intersections draw the line *x y*, and this line will be the diameter of the least circular space into which all the rays that fall upon the lens, parallel to its axis, can be collected. And this circle, which for brevity's sake is called the circle of aberration, is the true focus of the lens or place where the image of the object is formed.

Let the sine of incidence going out of glass be  $n$ , the sines of refraction (into air) of the least and the most refrangible rays be  $p$  and  $q$ ; then if a plano-convex lens be exposed with the plane side to the sun, the diameter of the circle of aberration  $xy$  (or image of the sun formed of rays of different refrangibility) is to the diameter of the lens  $AB$ , as  $q - p$  to  $q \times p - 2n$ .\*

\* This theorem is well known to mathematicians.



From this given ratio of  $AB$  to  $xy$  it follows, that the image of the sun within a telescope varies with the aperture of the object glass.

*Sir Isaac Newton* found by most accurate experiments that where the sine of incidence was 50, the sines of refraction of the red and the violet rays were 77 and 78. Hence  $q-p$  is to  $q+p-2n$  as 1 to 55. And therefore  $AB : xy :: 55 : 1$ ; or

$$xy = \frac{AB}{55}.$$

To elucidate this theorem by examples, let the diameter of the object glass of a telescope, which is 4 inches, be contracted to 3 inches, and afterwards to 2; then the diameters of the circles by aberration formed by parallel rays, will be  $\frac{4}{55} = .072$ ,  $\frac{3}{55} = .054$ , and  $\frac{2}{55} = .036$  respectively.

*The same Property of Vision demonstrated otherwise.*

Thus, let  $n$  represent the sine of incidence, and  $p$  and  $q$  the sines of refraction, as before.

The sine of incidence of every ray, is to its sine of refraction in a given ratio.\*

And the sine of incidence of the extreme ray  $PA$ , varies with the aperture of the lens. For  $n$  becomes less as  $PA$  approaches the axis of the lens  $Ev$ .

Therefore the sines of refraction  $p$  and  $q$ , the angle  $xAy$ , and its subtense  $xy$  increase or decrease with  $AB$ . Consequently the image of the sun or moon, upon the retina increases or decreases in magnitude with the pupil of the eye.

Deduction;

Now as the rays of artificial light are differently refrangible, it is evident from the given ratio of  $AB$  to  $xy$ , in which they increase or decrease at the same time that the image of a candle formed in the focus of a convex lens decreases with the aperture of the glass. For the rays of the sun and the light of a candle are both governed by the same law, in the formation of images in the focus of a lens; but this law does not obtain in the same degree in both objects, in consequence of the rays of the latter being in a more diverging state than those of the former.

—applied in support of the author's experiments.

Hence the truth of the result of my experiments, which were published in Vol. IX. page 164 of this Journal, is proved from the discoveries of *Sir Isaac Newton*.

\* *Newton's Optics*, page 64.



What I have further to advance on this subject must be reserved until some future opportunity, as it would exceed the limits of this paper.

E. WALKER.

Lynn, May 16, 1805.

#### IV.

*New Method of decomposing the Sulphate of Barytes for preparing the Muriate of that Earth, and Preparation of the Muriate. By Mr. GOETTLING.\**

THE muriate of barytes is now in such general use, that every improvement in the mode of preparing it must meet a favourable reception. This will render the new method of Mr. Goettling acceptable to the public.

The decomposition of sulphate of barytes by means of charcoal requires a strong fire continued a long time, and never succeeds completely. This is owing on the one hand to the strongly oxygenated quality of the acidifying principle in the sulphuric acid, so that in its translation to the charcoal it gives out but little caloric; and on the other hand to the difficulty of imparting a certain degree of heat to a mixture, into which a large quantity of a body that is so bad a conductor of heat as charcoal enters. To remedy the first of these defects, I had already proposed to increase the proportion of charcoal a little, and to incorporate with the mixture of charcoal and sulphate of barytes a twentieth of nitrate of potash. To remedy the second, Mr. Goettling advises to add muriate of soda to the mixture, which serves at the same time as a conductor of heat and a flux. The following is his method.

Four parts of native sulphate of barytes in fine powder are to be mixed with one part of muriate of soda and half a part of charcoal powder. This mixture is to be pressed hard into a Hessian crucible, and exposed for an hour and half to a red heat in a good wind furnace. After it has grown cold, the

Muriate of barytes much used.

Decomposition of sulphate of barytes by charcoal troublesome and incomplete.

Remedies: to use more charcoal and nitrate of potash; —and muriate of soda.

Mr. Goettling's process. Sulphate of barytes with muriate of soda and some charcoal are heated together.

\* Translated from Van Mons's *Journal de Chimie*, Vol. VI. p. 80. Originally published in the *Taschen-Buch fuer Scheidekuenstler*.

The saline mass is then to be dissolved. mass is to be reduced to a coarse powder, and boiled for a moment with sixteen parts of water. The liquor is then to be filtered, and kept in well stopped bottles.

The time of exposure to heat may be shortened to one half, if the quantity of muriate of soda be doubled, and the matter occasionally stirred. In this case too, double the quantity of water should be used to lixiviate the mass.

An addition of muriatic acid expels the sulphureous acid and leaves muriates of barytes and of soda.

To prepare muriate of barytes with this lixivium of sulphuret of barytes, which at the same time holds in solution muriate of soda, muriatic acid is to be added in separate portions, till sulphurated hydrogen gas is no longer extricated. The liquor is then to be filtered, a little hot water is to be poured on the residuum, and the liquor is to be evaporated to a pellicle. The lixivium being then filtered afresh, is to be set to crystallize; the muriate of soda, which is much more soluble in water than the muriate of barytes, and not more soluble with heat than without, is not deposited by cooling, and the muriate of barytes crystallizes alone.

These crystals of muriate of barytes are separated by cooling after evaporation.

The remaining lixivium is to be evaporated and set to crystallize again, and this is to be repeated till no more crystals of muriate of barytes are formed.

The barytic salt is perfectly white and pure.

The barytic salt thus obtained, if care be taken not to employ an excess of muriatic acid, is perfectly white, on account of the hydrosulphuret, by which the iron and other metallic substances are precipitated. To be more certain that it contains no muriate of soda, the different products of the crystallization should be mixed together, dissolved, and re-crystallized.

## V.

*On the Action of Platina and Mercury upon each other.* By RICHARD CHENEVIX, Esq. F. R. S. M. R. I. A. &c. From the *Philosophical Transactions* for 1805, p. 104.

*Freyberg, June 3d, 1804.*

Reference to the author's paper on palladium.

ON the 12th of May, 1803, I had the honour of presenting a Paper to the Royal Society,\* the object of which was to discover the nature of palladium, a substance just then announced

\* Inserted in our Journal, Vol. VII. 85, 176.



to the public as a new simple metal. The experiments which I had made for this purpose led me to conclude that palladium was not what it had been stated to be, but that it was a compound of platina and mercury.

It was natural to suppose that a subject so likely to spread its influence throughout the whole domain of chemistry, and which tended even to the subversion of some of its elements, would awaken the attention of philosophers. We find accordingly, that it has become a subject of enquiry in England, France, and Germany; but the experiments which I had recommended as the least likely to fail, have been found insufficient to insure the principal result; and I have had the mortification to learn that they have been generally unsuccessful. I have even reason to believe that the nature of palladium is still considered by chemists, at least with a very few exceptions, as unascertained; and that the fixation of mercury by platina is by many regarded as visionary.

The first doubts were manifested in England; and Dr. Wollaston very early denied the accuracy of my inquiries. But as he has not published his experiments, I have had no opportunity of discussing them. His opinion, however, must have such weight in the learned world, that I should have neglected a material fact in the history of palladium, if I had not mentioned it in this place.

In France the compound nature of palladium has been more generally credited. When the National Institute was informed of my experiments, a report was ordered to be made upon them, and M. Guyton was the person appointed for the purpose. He repeated some of the experiments, and produced some of his results. His general conclusion was the same as mine.

Messrs. Vauquelin and Fourcroy then undertook the subject, and they were led by it to the confirmation of the recent discovery of Mons. Descotils. The existence of a new metal, which that chemist had found in crude platina, received great sanction from their experiments; and thus the discussion upon palladium has established a fact which will be considered as interesting, but which would be much more so, were we not already overburthened with substances which our present ignorance obliges us to acknowledge as simple.



—infer that palladium contains no mercury; but is platina with the new metal of Descotils.

No sooner were these celebrated chemists convinced of the existence of a new metal in platina, than they concluded that it must play a principal part in the composition of palladium. Shortly after this, in a note to a letter from M. Proust to M. Vauquelin, in which M. Proust expresses his astonishment concerning all he has read upon palladium, Mess. Fourcroy and Vauquelin further declare, as their opinion, that this compound metal does not contain mercury, but is formed of platina and the new metal. Whether this new substance does or does not play a principal part in the formation of palladium, could not be ascertained at the time my experiments were made, because the new metal itself was not then known. But from all that Mess. Fourcroy and Vauquelin have stated, in such of their different memoirs upon this subject as I have seen, the grounds of their supposition have not appeared. May we not refer their opinion, then, to that common propensity of the mind, against which M. Fourcroy has himself warned us with equal justness and eloquence on another occasion, namely, a proneness to be allured by novelty beyond the bounds of rational belief, and to convert principles which are new into principles of universal influence.

Mess. Rose and Gehlen,

Mess. Rose and Gehlen\* were the first among the German chemists who instituted experiments upon palladium; and M. Richter has also published a paper on the same subject.

—attempted without success to form palladium by precipitating a mixed solution of platina and mercury by gr. sulphate of iron;

The first attempt of Mess. Rose and Gehlen to form palladium was by the precipitation of a mixed solution of platina and mercury by green sulphate of iron. Their result was precisely that which I had observed when my operations failed altogether, and which of course was the most frequent. This method was repeated twice. The second time the precipitate of platina and mercury was boiled with muriatic acid, in order to free it from iron; but the latter trial was not more successful than the former.

—and also by passing sulphuretted hydrogen through the mixed solution;

Their third experiment was, what they have called, a repetition of that in which I had obtained palladium by passing a current of sulphuretted hydrogen gas through a mixed solution of platina and mercury. Their method was the following.

\* Neues Allgemeines Journal der Chemie herausgegeben von Hermstadt, Klaproth, Richter, Scherer, Tromsdorff, und Gehlen. Ersten bandes funftes heft.

They

They dissolved one hundred and fifty grains of platina with four hundred and fifty of mercury, and added a solution of hydrosulphuret of potash. They obtained a precipitate which, at first, was black, afterwards gray; but the whole became black by being stirred. To be certain that all the metal was precipitated, they added an excess of sulphuret of potash, and perceived that a part of the precipitate was re-dissolved. The liquor was then filtered, and to that part of it, which contained the re-dissolved precipitate, an acid was added. From this process they obtained a yellow precipitate weighing ninety-one grains; and fifty grains of this exposed to a strong heat, left three-eighths of a grain of platina. They obtained no palladium from that part of the precipitate which had not been re-dissolved; and the result of the experiment was complete failure.

by adding hydro-sulph. of potash to the m. solution;

—but they obtained no palladium.

I shall not make any observation upon the issue of this process, since, in this case, the best conducted is but too liable to be unsuccessful, and that without any apparent fault in the operator. But as it has been given as a repetition of one of mine, it may not be fruitless to examine how far the repetition was exact.

I had passed a current of sulphuretted hydrogen gas through a mixed solution of platina and mercury, by which means they were precipitated together. My object was so intimately to combine sulphur with these metals, that when exposed to heat, they might (if I may be allowed the expression) be in chemical contact with it at the moment of their nascent metallic state; and as a low temperature suffices, as well to reduce those metals, as to combine palladium with sulphur, I hoped that those effects might be produced before the total dissipation of the mercury. How far my expectation was fulfilled has been stated in my former paper.

They did not exactly repeat the author's experiment. He used the pure gas; they added an alkali;

The sulphuretted hydrogen gas which Messrs. Rose and Gehlen presented to those metals was combined with potash. Now, in the course of docimastic lectures annually delivered by M. Vauquelin at the Ecole des Mines in Paris, when he was Professor at that establishment, it was his constant custom to exhibit an experiment to prove that mercury, precipitated from its solution by many of the alkaline and earthy hydrosulphurets, was redissolved by adding an excess of them.

—and this last substance acting not only on mercury;

It is to be observed that the experiment was not repeated.



—but on the platina;

—would have an effect nearly opposite to that of the author's process.

They could not fuse platina.

The author used borax.

Particular account of his process.

A Hessian crucible was lined with lamp black;

It is moreover well known, that there is a strong affinity between potash and the oxide of platina; and also that when those substances are brought together in solution, a triple salt, but little soluble, is the result. It was to avoid these difficulties that I had employed uncombined sulphuretted hydrogen gas; for the method adopted by Mess. Rose and Gehlen appearing to me to be the application of two divellent forces, I presumed that it would produce a separation. The result of their experiment, which, it appears from their paper, they had not anticipated, shews the necessity of the precaution I had used. The operation which they performed to unite platina and mercury was, in fact, nearly the reverse of that which they supposed they had repeated from me, and might have been applied perhaps with a better prospect of success towards the decomposition of palladium.

Mess. Rose and Gehlen seem, in many parts of their paper, to question my having fused platina; and inform us that although they had exposed this metal in the furnace of the Royal Porcelain Manufactory of Berlin, in which Wedgewood's pyrometer ceased to mark the degree of heat, they could not accomplish its fusion. Many of my friends in England have however seen the buttons which I obtained, and which were not few in number. The flux which I had used was borax. But no mention is made in any one of the operations of Mess. Rose and Gehlen of borax having been employed.

In many of their attempts they obtained an irregular and porous mass, which of course was of a specific gravity much inferior to that of platina; and it might be inferred from their paper that the diminution of specific gravity, which I had observed, was owing to the same cause. It is true, not only that I had very often obtained such a mass, but that I had frequently also observed no diminution whatsoever in the specific gravity of the button which resulted from my operations. But all those upon which I had founded the conclusions alluded to by Mess. Rose and Gehlen were performed in the following manner, and have been repeated since. A Hessian crucible was filled with lamp-black, and the contents pressed hard together. The lamp-black was then hollowed out to the shape of the crucible as far as one-third from the bottom, leaving that much filled with the compressed materials; this lining, which adhered strongly to the sides of the crucible, was made extremely thin,

in



in order not to obstruct the passage of caloric. A cylindrical—a small cylindrical cavity was made in the lower part of the lining;  
 piece of wood, as a pencil, was then forced into the centre of  
 the thick mass of lamp-black at the bottom, and the diameter  
 of this rod was determined by the quantity of metal to be  
 fused, or varied according to other circumstances at pleasure.  
 In general the axis of the cylindrical hole was about three or  
 four times the diameter of the basis. After withdrawing the  
 rod the crucible was about half filled with borax. Upon this—the metal occupied the middle of a mass of borax that filled up the cavity of the vessel;  
 was placed the metal to be fused; and if it had been before  
 melted into a cylindrical form, the axis of the metallic cylinder  
 was placed horizontally, and was of course perpendicular to  
 the axis of the cylindrical excavation at the bottom of the  
 crucible. More borax was then added to cover the piece of  
 metal, and another quantity of lamp-black was pressed hard  
 over the whole in order to keep it tight together. An earthen  
 cover was finally luted to the crucible, and in this state it was  
 exposed to heat in a forge, in which upon another occasion, I  
 had, in the presence of Mess. Hatchett, Howard, Davy, and  
 others, completely melted a Hessian crucible lined and pre-  
 pared in the same manner. The fuel which I used was the—strong heat was applied;  
 patent coak of Mess. Davey and Sawyer. In the present ex-  
 periments I moderated the heat so as not materially to injure  
 the crucible, and upon taking it out of the fire, the lining was  
 generally found so compact and so firm that it remained in a  
 solid mass after the crucible was broken. When the metallic—and when the metal lost its first dimensions and occupied the cylindrical cavity it must have been fused.  
 cylinder occupied the space at the bottom, it was natural to  
 suppose that it had been fused; because in no other state but  
 that of liquidity could it have run into the mould. In order  
 however to prevent all objections I had the precaution to make  
 the hole of a different diameter from the metallic cylinder,  
 and to observe whether the necessary change in the shape of  
 the latter ensued. If after such a test, repeated as often as  
 required, I perceived that the metal did not vary in its specific  
 gravity, I thought myself authorised to conclude that it was  
 exempt from air.

M. Richter says that he had hoped to have put himself in  
 possession of a considerable piece of palladium by repeating  
 with minute accuracy the process which I had recommended  
 as the best. He precipitated a mixed solution of platina and  
 mercury by a solution of green sulphate of iron; and after  
 varying the subsequent operations, to which he submitted the  
 product

M. Richter's attempt and conclusions;

product he had obtained by this method; he was led to the following important conclusions amongst others of less consequence. 1st, That two metals, the separate solutions of which are not acted upon by a third body, may be acted upon, and even reduced to the metallic state, by that same body when presented to them in one and the same solution.

2dly. That mercury is capable of entering into combination with platina so, that it cannot afterwards be separated by fire. From the first of these conclusions it is evident, that metals in their metallic state are not incapable of chemical action upon each other; and from the second, that mercury can be fixed (it is purposely that I use the alchemical expression) by platina.

Attempts of Tromsdorff and Klaproth: not successful.

In addition to the chemists abovementioned, I must name two more who in Germany have been occupied by palladium. M. Tromsdorff, in a letter to the authors of the journal already quoted, mentions his having made some fruitless attempts to form this combination; and Klaproth, in a letter to M. Vauquelin published in the *Annales de Chimie*, for Ventose, an 12, likewise says that he could not succeed in producing palladium.

Remarks. The chemists who expected to succeed in a few trials, could not have attended to the author's paper:

Messrs. Rose and Gehlen, as well as M. Richter, had conceived from my Paper a reliance on the success of their experiments, which no words of mine had authorised, and have accused me of enforcing the truth of my results with a degree of certainty which their observations do not countenance. M. Richter supposed that the formation of palladium was attended with no difficulty; and in general they have laid so much stress upon this charge, that I should be inclined to think my Paper had not been read by these chemists. In referring to it again, I find there is hardly a page in which I do not mention some failure, and no experiment, of the very few which occasionally succeeded, is related without my stating at the same time that it was repeatedly unsuccessful. As far as regards palladium, it is rather a narration of fruitless attempts than a description of an infallible process, and more likely to create aversion to the pursuit than to inspire a confidence of success. The course of experiments which I had made, as well before as after reading my Paper to the Society, took me up more than two months, and employed me from twelve to sixteen hours almost every day. I had frequently

He made many hundreds of experiments, and succeeded completely but four times.

seven



seven or eight operations in the forge to perform daily, and I do not exaggerate the number of attempts I made during this time, as well in the dry as in the humid way, in stating them to have been one thousand. Amongst these I had four successful operations. I persevered, because even in my failures I saw sufficient to convince me that I should quit the road to truth if I desisted. After all my labour and fatigue I cannot say that I had come nearer to my object, of obtaining more certainty in my processes. Their success was still a hazard on the dice, against which there were many chances; but till others had thrown as often as I had done, they had no solid right to deny the existence of such a combination. On this foundation none, I believe, have established such a right. Mess. Rose and Gehlen do not say how often their experiments were repeated; but it is probable that if they had been performed very often, these authors would not have neglected to mention it. M. Richter states his merely as preparatory to more extensive researches; and M. Tromsdorff, as well as M. Klaproth, mention little more than the fact. If the German chemists have concluded against my results, they have done so without just grounds, and without having bestowed upon them that labour and assiduity for which they are usually so remarkable.

In this state of uncertainty the compound nature of palladium received an indirect, but a very able, support from some experiments of M. Ritter, the celebrated Galvanist of Jena. M. Ritter had ascertained the rank which a greater number of substances hold in a galvanic series, arranged according to the property they possess of becoming positive or negative when in contact with each other. He had established the following order, the preceding substance being in a *minus* relation to that which comes next. Zinc, lead, tin, iron, bismuth, cobalt, antimony, platina, gold, mercury, silver, coal, galena, crystallized tin ore, kupfer nickel, sulphur, pyrites, copper pyrites, arsenical pyrites, graphite, crystallized oxide of manganese. He had the goodness to try palladium in my presence, and found it to be removed, not only from what I believed to be its constituent parts, but altogether from among the metals, and to stand between arsenical pyrites and graphite. This result led Mr. Ritter into a new and general train of reasoning, and induced him to undertake the examination of a

M. Ritter the great galvanist, has established the order of galvanic relation in bodies.

He found palladium to be removed in that order from among the metals to the compound bodies.

great



Other interesting facts.

great number of alloys, and of a variety of amalgams. He considered the subject as a philosopher; and his operations were those of a consummate experimentalist. It would be doing him an injustice to attempt an extract of his ingenious paper, which contains a series of the most interesting experiments. I shall merely observe for the present purpose, that it very rarely happened that the mixture of two metals bore any determinate relation to the same metals when separate; that in every case the smallest variation in the proportions produced the most marked effects; and that M. Ritter has furnished us with an instrument calculated to detect the presence of such small quantities as have hitherto been considered as out of the reach of chemistry. As palladium presents a very striking instance of the anomaly, to which all compounds seem to be more or less subject, by being removed altogether from the series of simple metals, this may serve to support the other proofs of its compound nature.

The objectors to the author's conclusions have not paid attention to the repeated failures of all his methods.

One of the principal objections of those who dispute the truth of my conclusions with respect to palladium, is grounded upon the repeated failure of all the methods I had made use of in forming it; but this cannot be of very great weight, when we consider the uncertainty of many other operations of chemistry. The most simple are sometimes liable to fail: and the easiest analyses have often given different products in the hands of different chemists, who yet enjoy indisputable and equal rights to the title of accuracy. The progress which we have made in some parts of the science has not removed the obstacles which impede our advancement in others. We have no method of proving the truth of an experiment except by repeating it; yet this often tends to show nothing more than contradictory results, and consequently the fallibility of the art.

A recent case in chemistry which is perfectly analogous to this.

But a recent case has occurred which is perfectly analogous to that of palladium. A few years ago Professor Lampadius, in distilling some substances which contained sulphur and charcoal, obtained a liquid product of a peculiar nature. He repeated his experiments, but in vain: and after many fruitless attempts abandoned his researches, and confined himself to stating the fact to the chemical world. Little notice was taken of it, and not much interest excited by an experiment so likely to fail. Some time after this Messrs. Clement and

Deformes

Desormes obtained the same result, and attempted to produce the substance a second time. They performed a vast number of experiments; but their success bore no proportion to their diligence and zeal. They published an account of their process and its consequences, but gained little credit, as no person was fortunate enough to produce the same substance. Many disbelieved the experiments altogether, and denied the existence of such a combination; whilst others, less inclined to doubt, attributed its formation to fortuitous circumstances which might never again occur together. In February, 1804, Professor Lampadius, in distilling some pyritized wood, though with a different intent, obtained the same substance. As he had it now in his power to observe the phenomena that attended its formation, he discovered, and has communicated to the world, a method of producing it, which never fails. Since his late paper upon the subject, as the necessary precautions can be followed by every chemist, Messrs. Clement and Desormes have obtained that credit to which their experiments had, in truth, always been entitled; and the formation, of what Professor Lampadius terms his sulphur-alcohol is no longer a result of chance, or accounted for by being supposed one of those subterfuges to which human pride resorts, in order to spare itself the confession of human weakness.

Discovery of the sulphur-alcohol of Lampadius, which he could not repeat: It was afterwards once only verified by Clement and Desormes.

The inventor re-discovered the substance, and it can be produced at pleasure.

The observation of any new fact becomes a matter of general concern, and truly worthy of philosophic contemplation, then only, when its influence is likely to be extended beyond the single instance to which it owes its discovery. Whether water were a simple body or a compound could have been of little importance as an insulated fact; but, connected with the vast chain of reasoning it gave rise to, it opened a new field for genius to explore. If in the present case our researches were to be confined merely to ascertaining whether palladium were a simple metal or a compound, all the advantages likely to arise from the facts observed during the inquiry would be lost; and an object of the most comprehensive interest would thus sink into a controversy concerning the existence of one more of those substances, which we have dignified with the name of elements. It was in this point of view that Messrs. Richter and Ritter considered the subject as far as they went, and

New facts are of value the more extensive their relations, &c.

a few



a few facts are stated in my first Paper in support of the opinion, that palladium is but a particular instance of a general truth.

Conclusion :  
that compound  
metals may be  
taken for simple,  
and the detection  
be extremely  
difficult.

By taking the reasoning on this subject then, in its widest extent, we shall be led, I think, to the following conclusion : That metals may exercise an action upon each other, even in their metallic state, capable of so altering some of their principal properties as to render the presence of one or more of them not to be detected by the usual methods. In this is contained the possibility of a compound metal appearing to be simple ; but to prove this must be a work of great time and perseverance ; and can only be done by considering singly and successively the different cases which it contains, and by instituting experiments upon each. When an affinity which unites two bodies, and so blends their different properties as to make them apparently one, has taken its full effect, it will not be easy to separate them ; and this will be more particularly the case when neither of those substances is remarkable for exercising a powerful action upon others. The method of analysis therefore does not promise much success ; and the labour of synthesis is sufficient to deter any individual from the undertaking.

Platina and mercury do act on each other, and disguise the properties of each.

It is my intention now to exhibit one example of my position, and to prove that platina and mercury act upon each other, in such a manner as to disguise the properties of both. I shall therefore wave for the present all consideration of palladium, which is in fact but a subordinate instance of the case before us.

Example: Platina is precipitated in the metallic state by green sulph. of iron, if mercury or silver be present ; but not else.

When a solution of green sulphate of iron is poured into a solution of platina, no precipitate, nor any other sensible change ensues. This I had already observed, and it has since been confirmed by all who have written upon the subject. But, if a solution of silver or of mercury be added, a copious precipitate takes place. This precipitate contains metallic platina and metallic silver or mercury ; some muriate of one or other of the latter metals is also present, as it is not easy to free the solution of platina from all superfluous muriatic acid. But these salts are of no importance in the experiment, and can be separated by such methods as a knowledge of their chemical properties will easily suggest. The proper object of consideration is the reduction of the platina to the metallic state, which



which does not happen when it is alone. I have tried to produce the same effect with other metals and platina, but I have not observed any thing similar. It is therefore fair to conclude, that when a solution of platina is precipitated in a metallic state by a solution of green sulphate of iron, either silver or mercury is present.

The precipitation of a mixed solution of platina and silver requires no further caution than to free the salt of platina as much as possible from muriatic acid; for as I observed in my former Paper, the effect of nitrate of silver poured into muriate of platina, is to produce a precipitate, not of muriate of silver, but of a triple muriate of platina and silver. It was by this experiment that I then proved the affinity of these two metals; for when silver is not present, muriate of platina is among the most soluble salts. The best method of presenting the three solutions of platina, silver, and green sulphate of iron to each other, is first to pour the filtered solution of the last into the solution of platina, and then, after mixing them thoroughly together, to add the solution of silver by degrees, and to stir them constantly. In this, as in all similar operations, the presence of all acids, salts, &c. excepting those necessary for the operation, should be avoided; and if proper proportions have been used, and all circumstances attended to, the precipitation of these two metals will be very complete.

Nitrate of silver added to mur. platina throws down a triple muriate of pl. and silver.

Precise method of precip. pl. and silv. in the metallic state by sulph. iron.

But the precipitation by a solution of mercury requires to be further considered, as the state of oxidizement of this metal, as well as the acid in which it is dissolved, produces a considerable modification in the result. In the first place the oxide, at the minimum of oxidizement, dissolved in muriatic acid, is unfit for the experiment; and even the red oxide dissolved in the same acid, or corrosive sublimate, is not the most advantageous. When a warm solution of the latter is poured into a mixed solution of platina and green sulphate of iron also warm, as in the case of silver, these substances are brought into contact under the most favourable circumstances. Yet even thus the precipitation is slowly and imperfectly formed, often not till several hours have elapsed; and sometimes a very great deficiency of weight is observed, between the quantities used and those recovered directly by this method. If a solution of nitrate of mercury be used, the effect is produced

The same, but with solution of mercury.

more

more rapidly, and the precipitate is more abundant. The precipitation of muriate of platina by nitrate of silver, and the combination which ensues from it, suggested to me an experiment which I must state at length, as from the result of it consequences are deduced which modify some of the experiments of my former Paper.

Mercury dissolved at the minimum of oxidation will fall in a triple salt with platina without any addition of sulph. iron.

It occurred to me that a method of uniting platina and mercury without the intervention of any other metal, or of any substance but the solvents of these metals might be accomplished as in the case of silver and platina. I therefore poured a solution of nitrate of mercury, which solution being at the minimum of oxidizement, consequently formed an insoluble muriate with muriatic acid, into a solution of muriate of platina. The result was a triple salt of platina and mercury, which when the mercury was completely and totally at the minimum of oxidizement was nearly insoluble. To procure it in this state it is sufficient to put more metallic mercury into dilute nitric acid than the nitric acid can dissolve, and to boil them together. This triple salt of platina and mercury shall be presently examined: From this it is evident than to produce the union of platina and mercury, the latter being at its minimum of oxidizement in nitric acid the addition of green sulphate of iron is superfluous.

Not so if the mercury be at the maximum of oxidizement.

But if mercury be raised to its maximum of oxidizement in nitric acid the case is different, for no precipitation occurs till the green sulphate of iron is added. The most advantageous method for precipitating platina and mercury by green sulphate of iron is, I believe, the following. Mix a solution of platina with a solution of green sulphate of iron, both warm, and add to them a solution of nitrate of mercury at the maximum of oxidizement also warm. It is necessary to avoid excess of acid, salt, &c. in this as in all such cases. With due care the precipitation of both metals will then be complete.

Whether mercury be precipitable, singly, by green sulphate of iron? &c.

By comparing the experiments made with mercury and platina with those made with silver and platina, a striking resemblance will be found. This induced me to pursue the analogy, and to examine whether, independently of the action of platina, mercury had not the same property of being precipitated by green sulphate of iron as silver. Nitrate of silver is precipitated by green sulphate of iron, but muriate of silver is

not



not sensibly acted upon by the same reagent. The insolubility of muriate of silver might be alledged as the cause of this, if I had not tried the experiment by pouring nitrate of silver into green muriate of iron, in which case all the substances were presented to each other in solution. The result was not reduction, but muriate of silver and nitrate of iron. This fact rests upon a much more extensive basis than mere mechanical circumstances; and, if pursued with the intention it deserves, it would lead us into the wide expanse of complicated affinities and their relations. From reasoning alone we should be disposed to think that an acid, so easily decomposed as the nitric, would be sufficient to prevent the reduction of a metal which it can dissolve. But on the one hand it can spend its oxygen upon a part of the oxide of the green sulphate of iron, while on the other its affinity for oxide of silver is not powerful enough to retain it, when there is another part of the oxide of iron present to deprive it of oxygen. But the affinity of muriatic acid for oxide of silver, one of the strongest at present known, is sufficient to counterbalance all the other forces. There are many other instances of the same kind.

If then a solution of green sulphate of iron be brought into contact with either soluble or insoluble muriate of mercury, no reduction takes place; but if mercury, whether at the maximum or the minimum of oxidizement, be dissolved in nitric acid, and green sulphate of iron be added, the mercury is precipitated in the metallic state.

Mercury in nitric acid is precipitated (metallic) by gr. sulph. of iron.

These experiments are much stronger examples than the former of the effects produced by complicated affinities. They are of importance not only as objects of general consideration but in their application to the present subject. They most materially modify and are indispensable to the accuracy of the results I formerly stated; but I was not aware of them at the time I first engaged in the investigation of this subject. I can also now explain a very material difference between some proportions observed by M. Richter and myself in an experiment which that chemist had made as a repetition of one of mine.

These remarkable facts modify the results formerly stated.

I had poured a solution of green sulphate of iron into a solution of 100 parts of gold and 1200 of mercury, and had obtained a precipitate consisting of 100 of gold and 774 of mercury. M. Richter repeated, as he terms it, this experiment;

Precipitation of gold and mercury by gr. sulphate of iron: repeated by Richter,



ment; that is, he used 100 of gold and 300 of mercury, and obtained a precipitate weighing 102. He is surprised at the difference of weight between our results, which might be owing to his *method of repeating* the experiment; but the real cause of this difference lies, as I suppose, in my having accidentally used nitrate instead of muriate of mercury. I had never observed that with mercury and silver this operation had failed, and it must have been, because, on account of the known effect of muriatic salts upon those of silver, I had naturally avoided using a muriate of mercury,

The state of oxidation of the nitrate of mercury used with the solution of gold is of consequence: If the minimum of oxidation prevails, calomel and metallic gold fall down;

But the state of the nitrate of mercury which is used with a solution of gold is not indifferent. As green sulphate of iron reduces mercury when dissolved in nitric acid, as well as gold, it is necessary to mix the solutions of those metals before the green sulphate of iron is added, in order that both may be acted upon together. If the nitrate be at the minimum of oxidation, a precipitate is immediately formed upon mixing the solutions of gold and mercury. Calomel is produced by the muriatic acid of the solution of gold and the oxide of mercury; whilst the gold is reduced to the metallic state by a portion of the oxide of mercury becoming more oxidized, and forming the soluble muriate. The precipitate consists of calomel, of metallic gold, and of a very small portion of mercury which I believe to be in the same state; my reason for thinking so, is, that I have often observed, that a glass vessel in which I had sublimed some of it, was lined with a thin gray metallic coat.

but if the maximum, then nothing falls till the green sulphate of iron is added.

If, on the contrary, a nitrate of mercury be highly oxidized, no precipitate nor reduction of gold takes place until the green sulphate of iron is added. But at any rate the precipitation of gold and mercury, or of silver and mercury by green sulphate of iron, cannot be adduced as an argument to support the affinity of these metals, since the effect is the same, whether they are separate or united.

These preliminary considerations were necessary as well for the rectification of my former experiments as for the pursuit of my present object; and now to return to platina.

Experiments with platina.

1. Much of the highly oxid. sol. of mercury poured into a mixed solution of plat.

*Exper. 1.* If a solution of highly oxidized nitrate of mercury be poured into a mixed solution of platina and green sulphate of iron, the first action which takes place passes between the muriatic acid of the solution of platina and the oxide of mercury, by which a muriate of mercury is formed, but retained

in

in solution. This effect makes it advantageous to use a greater quantity of the solution of mercury than is merely capable of drawing down the given quantity of platina along with itself in the form of a metallic precipitate. When this precipitate is washed and dried, it will be found to weigh much more than the original quantity of platina; and the augmentation of weight has no limit but those of the mercury and the green sulphate of iron employed. But even after nitric acid has been boiled for a long time and in great quantities upon this precipitate, until it no longer dissolves any part of it, there still remains more undissolved matter than the original weight of the platina used in the experiment. By exposure to heat little more is left in general than the original platina; and sometimes even a diminution may be observed; for as the experiment is not attended with uniform success, it does not always happen that the whole of the platina is precipitated, but a portion of it will sometimes resist the action of the green sulphate of iron, even when sufficient mercury has been used.

and gr. sulph. of iron, throws down platina and mercury united.

Part of the latter is defended from nitric acid.

Heat drives off the mercury.

Before the precipitate has been exposed to heat it is dissolved more easily than platina by nitro-muriatic acid; and the solution when nearly in a neutral state gives a copious metallic precipitate, (yet not equal to the quantity employed,) when boiled with a solution of green sulphate of iron.

The compound precipitate is more soluble than plat. in n. m. acid, and gives much metallic precip. by add. of s. of gr. f. of iron.

*Exper. 2.* When a mixed solution of platina and mercury is precipitated by metallic iron, a quantity equal to the sum of the former metals is generally obtained. After nitric acid has been boiled for a long time upon the precipitate so formed, the original weight of platina, together with a considerable increase, remains behind, nor can nitric acid sensibly diminish it. It yields more easily than platina to the action of nitro-muriatic acid, and its solution in that acid, when neutralized, gives a precipitate, as in the former experiment, by green sulphate of iron. If this precipitate be exposed to a strong heat after it has been boiled with nitric acid, it loses a great part of its weight, and the platina alone will generally be found to remain.

*Exp. 2.* Metallic iron throws down the pl. and merc. from a mixed solution. Nitric acid does not deprive this metal of all its mercury. It is more soluble in n. m. acid; and precip. by gr. f. of iron. Heat usually expels the mercury from this.

*Exper. 3.* When a quantity of ammoniacal muriate of platina is treated according to the method of Count Muffin Pushkin to form an amalgam, and, after being rubbed for a considerable time with mercury, is exposed in a crucible to a heat gradually increased till it becomes violent, a metallic powder remains in

*Exp. 3.* Amalgam (from ammon. mur. of plat.) strongly heated, leaves a powder, acted on by n. m. acid, and copiously



precip. by gr. f. of iron.

the crucible. This powder is acted upon by nitro-muriatic acid, and when the solution is neutralized, a copious precipitate is formed upon the addition of green sulphate of iron. This effect takes place even after the metal has been fused in the manner described in the former part of this paper.

Exp. 4. Sulphur added in the last Exp. causes a greater precip.

Exper. 4. If sulphur be added to the ingredients recommended by Count Muffin Puschkin, and the whole treated as in the last experiment, the quantity of precipitate caused by green sulphate of iron in the nitro-muriatic solution of the button which results from the operation, is generally more considerable.

Exp. 5. Sulphur rubbed with am. m. of plat. easily melts. Mercury being added, strong fusion gives a button, sol. in n. m. acid and precipitable by gr. f. of iron.

Exper. 5. If sulphur be rubbed for some time with ammoniacal muriate of platina, and the mixture be introduced into a small Florence flask, it can be melted on a sand-bath. If mercury be then thrown into it, and the whole be well stirred together and heated, it may afterwards be exposed to a very strong fire and melted into a button. If this be dissolved in nitro-muriatic acid, it will give a precipitate, as in the former cases, by green sulphate of iron.

Exp. 6. The precip. from sol. of plat. and merc. by sulph. hydrogen being fused affords a sol. precip. by gr. f. of iron.

Exper. 6. If a current of sulphuretted hydrogen gas be sent through a mixed solution of platina and mercury, and the precipitate which ensues be collected, the metal may be reduced by heat; and with the addition of borax, it may be melted into a button which will not contain any sulphur. Green sulphate of iron causes a precipitate in the solution of this metal also.

Exp. 7. So likewise the precip. by phosphate of ammon.

Exper. 7. If to a mixed solution of platina and mercury, phosphate of ammonia be added; a precipitate takes place. If this be collected and reduced, it will be acted upon by green sulphate of iron poured into its solution, in the same manner as the metallic buttons in the preceding examples.

Exp. 8. Nitr. of merc. at min. of oxidiz. precipitates mur. of pl. The metallic compound dissolved in n. m. acid is precipitable by gr. f. of iron.

Exper. 8. I have already mentioned that when a solution of nitrate of mercury, at the minimum of oxidizement, is poured into a solution of muriate of platina, a mercurial muriate of platina is precipitated. The supernatant liquor may be decanted and the residuum washed; if this be reduced and afterwards dissolved in nitro-muriatic acid, it will yield a precipitate with green sulphate of iron. This method appears to me to be the neatest for combining platina and mercury, as the action which takes place is independent of every substance except the metals themselves.

Exper.



*Exper. 9.* One of the most delicate tests that I have observed in chemistry is recent muriate of tin, which detects the presence of the smallest portion of mercury. When a single drop of a saturate solution of neutralized nitrate or muriate of mercury is put into 500 grains of water, and a few drops of a saturate solution of recent muriate of tin are added, the liquor becomes a little turbid, and of a smoke-gray colour. If these 500 grains of liquid be diluted with ten times their weight of water, the effect is of course diminished, but still it is perceptible. I had on a former occasion observed the action of recent muriate of tin upon a solution of platina. If a solution of recent muriate of tin be poured into a mixed solution of platina and mercury, not too concentrated, it can hardly be distinguished from a simple solution of platina. But if too much mercury be present, the excess is acted upon as mercury; and the liquor assumes a darker colour than with platina alone.

*Exp. 9.* Recent muriate of tin is the most delicate test of mercury. It does not indicate the mercury in a mixed solution of that metal and platina.

From all these experiments it is evident that mercury can act upon platina, and confer upon it the property of being precipitated in a metallic state by green sulphate of iron. By *Experiments 1 and 2*, it is proved, 1st, That platina can protect a considerable quantity of mercury from the action of nitric acid; and 2dly, That mercury can increase the action of nitromuriatic acid upon platina. From *Experiments 3, 4, 5, 6, 7, 8*, it appears that mercury can combine with platina in such a manner as not to be separated by the degree of heat necessary to fuse the compound, since after the fusion it retains that property, which is essentially characteristic of the presence of mercury in a solution of platina. The 8th *Experiment* proves that the action of mercury upon platina is not confined to the metallic state; but that these metals can combine and form an insoluble triple salt with an acid which produces a very soluble compound with platina alone. The 9th *Experiment* shows that platina can retain in solution a certain quantity of mercury, and prevent its reduction, by a substance which acts most powerfully to that effect, when platina is not present. That part of the general position therefore which is the object of this paper is proved, if these experiments, upon being repeated by other chemists, shall be found to be accurate.

Hence, 1, 2. Platina protects mercury from nitric acid; and mercury renders platina more subject to nitromur. acid: 3—8. Platina retains mercury in the strong heat of fusion. 8. Mercury and platina act on each other in saline composition. 9. Platina defends mercury in solution from being reduced by m. of tin.

One or two of the above experiments seem to be in contradiction to some that I have stated in my paper upon palladium;

Remark on palladium.

for in the present examples platina protects mercury against the action of nitric acid; whereas in palladium the mercury is not only acted upon itself, but it conduces to the solution of platina in the same acid. I am well aware of this objection; but confining myself to my present object, I shall wave all further discussion of it till another opportunity. In the mean time, however, it may be laid down as an axiom in chemistry, that the strongest affinities are those, which produce in any substance the greatest deviation from its usual properties.

The compounds of platina and mercury are subject to great variations, &c.

When a button of the alloy of platina and mercury as prepared by any of the above methods, is dissolved in nitromuriatic acid, and afterwards precipitated by green sulphate of iron, the entire quantity of the alloy used is seldom obtained. A considerable portion of platina resists the action of green sulphate of iron, and remains in solution. This may be looked upon as the excess of platina, and can be recovered by a plate of iron. Hence it appears that less mercury is fixed, than can determine the precipitation of the entire quantity of platina; yet in this state it can draw down a greater quantity of the latter, than when it is merely poured into a mixed solution of platina, not before so treated. Indeed the whole of these experiments tend, not only to show that these two metals exercise a very powerful action upon each other, but that they are capable of great variation in the state of their combination; and also that substances possessing different properties have resulted from my attempts to combine platina with mercury.

Investigation of the quantity of mercury thus fixed. It is supposed or inferred to be about 17 mercury to 83 platina, with the specific gravity 16.

This observation furnished me with a method of ascertaining, or at least of approaching to the knowledge of, the quantity of mercury thus fixed by platina, and in combination with it. The experiment, however, having been seldom attended with full success, I mention the result with the entire consciousness of the uncertainty to which it is subject. I observed the increase of weight, which the original quantity of platina had acquired in some cases, after it had been treated with mercury, and fused into a button. I counted that augmentation as the quantity of mercury fixed. I then determined how much was precipitated by green sulphate of iron from a solution of this alloy, and supposed it to contain the whole quantity of mercury found as above. But, even if attended with complete success, there is a chemical reason which must make us refuse our assent to this estimate. It is possible, and not



not unlikely, that a portion of mercury may be retained in solution by the platina, as well as that a portion of the platina may be precipitated by means of the mercury. The mean result, however, was that the precipitate by green sulphate of iron consisted of about 17 of mercury, and 83 of platina, when the specific gravity was about 16.

With regard to palladium, lest it should be supposed that either my own observations, or those of others have given me cause to alter my opinion; I will add, that I have as yet seen no arguments of sufficient weight to convince me, in opposition to experiment, that palladium is a simple substance. Repeated failure in the attempt to form it I am too well accustomed to, not to believe that it may happen in well conducted operations; but four successful trials, which were not performed in secret, are in my mind a sufficient answer to that objection. By determining the present question we may overcome the prepossession conceived by many against the possibility of rendering mercury as fixed, at an elevated temperature, as other metals; we may be led to see no greater miracle in this compound than in a metallic oxide, or in water, and be compelled to take a middle path between the visions of alchemy on the one hand, and the equally unphilosophical prejudices on the other, which they are likely to create. In the course of experiments just now related, I have seen nothing but what tends to confirm my former results, yet the only means which I can, after all, prescribe for succeeding, is perseverance.

To ascertain whether the opinion of Mess. Fourcroy and Vauquelin, that the new metal was the principal ingredient in palladium had any just foundation, I observed the methods they have recommended for obtaining pure platina; but I did not perceive any difference in the facility with which either kind of platina combined with mercury.

I might have added some more experiments to corroborate the evidence I have adduced to prove my assertion of the fixation of mercury by platina; but Mess. Vauquelin and Fourcroy have promised the Institute of France a continuation of their researches, and M. Richter concludes his paper with saying that he will return to the subject. From the labours of such persons some great and important fact must issue; and I hope that the present subject will not be excluded from their consideration. The facts contained in this paper cannot be submitted

The author continues to think that palladium is a compound, since four successful experiments were made of forming it.

Platina, purified in Fourcroy and Vauquelin's method, is equally combinable with mercury.

These chemists and M. Richter have promised to pursue the subject.



submitted to too severe a scrutiny; and no judge can be more rigid or more competent than the very person who was the first to doubt my former experiments. But it is necessary to be observed by whoever shall think them worth the trouble of verifying, that even these experiments are liable to fail unless proper precautions are used: that I have never operated upon less than one hundred grains; and that the results, which I have stated, however simple they may appear, have been the constant labour of some weeks.

### POSTSCRIPT.

Dr. Wollaston's palladium in crude platina proves nothing; and it may have been the product of the amalgamating process.

Since this paper was written Dr. Wollaston has published some experiments upon platina. He has found that palladium is contained in very small quantities in crude platina. This fact was mentioned to me more than a year ago by Dr. Wollaston. I have not yet seen a copy of his paper; but I shall merely observe here that, whatever be the quantity of palladium found in a natural state, no conclusion can be drawn as to its being simple or compound. Nothing is more probable than that nature may have formed this alloy, and formed it much better than we can do. At all events, the amalgamation to which platina is submitted before it reaches Europe, is sufficient to account for a small portion of palladium.

### VI.

*Method of obviating the Necessity of Lifting Ships.* By Mr. ROBERT SEPPINGS, of Chatham Yard\*.

Great saving and advantage of suspending instead of lifting ships. **T**HE method here to be described of suspending, instead of lifting, ships, for the purpose of clearing them from their blocks; affords a very great saving to the public; and abridges two-thirds of the time formerly used in this operation. From the saving of time another very important advantage is derived, namely, that of enabling large ships to be docked, suspended, and undocked, the same spring tides. Without enumerating the inconveniencies arising, and, perhaps, injuries, which ships are liable to sustain, from the former

\* From the Transactions of the Society of Arts, who voted him the gold medal, 1804.

practice of lifting them, and which are removed by the present plan; that which relates to manual labour deserves particular attention; twenty men being sufficient to suspend a first rate, whereas it would require upwards of 500 to lift her. The situation which Mr. Seppings held in Plymouth-yard, attached to him, in a great degree, the shoring and lifting of ships, as well as the other practical part of the profession of a shipwright. Here he had an opportunity of observing, and indeed it was a subject of general regret, how much time, expense, and labour, were required in lifting a ship, particularly ships of the line. This induced him to consider whether some contrivance could not be adopted to obviate these evils. And it occurred to him, that if he could so construct the blocks on which the ship rests, that the weight of the ship might be applied to assist in the operation, he should accomplish this very desirable end. In September 1800, the shoring and lifting the *San Josef*, a large Spanish first-rate, then in dock at Plymouth, was committed to his directions; to perform which, the assistance of the principal part of the artificers of the yard was requisite. In conducting this business, the plan, which will be hereafter described, occurred to his mind; and from that time, he, by various experiments, proved his theory to be correct: the blocks, constructed by him, upon which the ship rests, being so contrived, that the facility in removing them, is proportionate to the quantity of pressure; and this circumstance is always absolutely under command, by increasing or diminishing the angle of three wedges, which constitute one of the blocks; two of which are horizontal, and one vertical. By enlarging the angle of the horizontal wedges, the vertical wedge becomes of consequence more acute; and its power may be so increased, that it shall have a great tendency to displace the horizontal wedges, as was proved by a model, which accompanied the statement to the Society; where the power of the screw is used as a substitute for the pressure of the ship.

Twenty men  
supply the place  
of 500.

History.

The contrivance  
is a block, com-  
pounded of two  
horizontal  
wedges, acting  
under an obtuse  
vertical wedge,

which by the  
ship's weight,  
can, if needful,  
displace the  
others.

Mr. Seppings caused three blocks to be made of hard wood agreeable to his invention, and the wedges of various angles. The horizontal wedges of the first block were nine degrees; of the second, seven; and of the third, five; of course, the angle of the vertical wedge of the first block was 162 degrees; of the second 166; and of the third, 170. These blocks, or wedges,

Experiments.

Angles of the  
wedges.



A sloop rested upon them, and shored up.

The horizontal wedges were then driven out,

by battering-rams,

supported on wheels.

Other experiments.

It was found that the weight of the vessel was capable of pressing out the wedges;—

wedges, were well executed, and rubbed over with soft soap for the purpose of experiment. They were then placed in a dock, in his Majesty's yard at Plymouth, in which a sloop of war was to be docked; on examining them after the vessel was in, and the water gone, they were all found to have kept their situations, as placed before the ship rested upon them. Shores in their wake were then erected to sustain the ship, prior to the said blocks being taken from under the keel. The process of clearing them was, by applying the power of battering-rams to the sides of the outer ends of the horizontal wedges; alternate blows being given fore and aft; by which means they immediately receded, and the vertical wedges were disengaged. It was observed, even in this small ship, that the block which was formed of horizontal wedges of nine degrees, came away much easier than those of seven, and the one of seven, than that of five. In removing the aforesaid blocks by the power of the battering-rams, which were suspended in the hands of the men employed, by their holding ropes passed through holes for that purpose, it was remarked by Mr. Seppings, that the operation was very laborious to the people; they having to support the weight of the battering-rams, as well as to set them in motion. He then conceived an idea of affixing wheels near the extremity of that part of the rams, which strikes the wedges. This was done before the blocks were again placed; and it has since been found fully to answer the purpose intended, particularly in returning the horizontal wedges to their original situations, when the work is performed for which they were displaced; the wheels also giving a great increase power to the rams, and decrease of labour to the artificers; besides which, the blows are given with much more exactness. The same blocks were again laid in another dock, in which a two-decked ship of the line was docked. On examination they were found to be very severely pressed, but were removed with great ease. They were again placed in another dock, in which a three-decked ship of the line was docked. This ship having in her fore-mast and bowsprit, the blocks were put quite forward, that being the part which presses them with the greatest force. As soon as the water was out of the dock, it was observed, that the horizontal wedges of nine and seven degrees had receded some feet from their original situations. This afforded Mr. Seppings a satisfactory proof, which experience has since demonstrated, (though



(though many persons before would not admit of, and others could not understand, the principle) that the facility of removing the blocks or wedges, was proportionate to the quantity of pressure upon them. The block of five degrees kept its place, but was immediately cleared, by applying the power of the battering-rams to the sides of the outer ends of the horizontal wedges. The above experiments being communicated to the Navy Board, Mr. Seppings was directed to attend them, and explain the principle of his invention; which explanation, farther corroborated by the testimonials of his then superior officers, was so satisfactory, that a dock was ordered to be fitted at Plymouth under his immediate directions. The horizontal wedges in this, and in the other docks, that were afterwards fitted by him, are of cast iron, with an angle of about five degrees and a half, which, from repeated trials, are found equal to any pressure, having in no instance receded, and, when required, were easily removed. The vertical wedge is of wood, lined with a plate of wrought iron, half an inch thick. On the bottom of the dock, in the wake of each block, is a plate of iron three quarters of an inch thick, so that iron at all times, acts in contact with iron.

and therefore  
that if made of  
the proper angle  
they would be  
secure, and  
always capable of  
being driven out.

The best angle is  
about  $5\frac{1}{2}$  degrees  
for cast iron  
wedges.

The placing the sustaining shores, the form and sizes of the wedges, and battering-rams, &c. also the process of taking away, and again re-placing, the wedges of which the block is composed, are also exemplified by a model.

The dock being prepared at Plymouth, in August, 1801, a large 80-gun ship suspended by this method, the Canopus, a large French 80-gun ship, was taken in, and rested upon the blocks; and the complete success of the experiment was such, that other docks were ordered to be fitted at Sheerness and Portsmouth dock-yards, under Mr. Seppings's directions. At the former place a frigate, and at the latter a three-decked ship, were suspended in like manner. This happened in December, 1802, and January, 1803; and the reports were so favourable, as to cause directions to be given for the general adoption of these blocks in his Majesty's yards. This invention being thought of national consequence, with respect to ships, but particularly those of the navy, government has been pleased to notice and reward Mr. Seppings for it.

A large 80-gun  
ship suspended  
by this method.

The time required to disengage each block, is from one to three minutes after the shores are placed: and a first-rate ship be disengaged in three minutes.

It is not required to suspend the ship in all cases. For the repairs may be done by successive removal of blocks.

The lifting of ships was a frequent operation in the navy.

This invention is of value in other undertakings.

Fid of a top-gallant mast,

applied by  
Capt. Wells,

Mancœuvre of striking the mast, &c.

on about fifty blocks. Various are the causes for which a ship may be required to be cleared from her blocks, viz. to shift the main keel; to add additional false keel; to repair defects; to caulk the garboard seams, scarps of the keel, &c. Imperfections in the false keel, which are so very injurious to the cables, can in the largest ship be remedied in a few hours by this invention, without adding an additional shore, by taking away blocks forward, amid-ships, and abaft, at the same time; and when the keel is repaired in the wake of those blocks, by returning them into their places, and then by taking out the next, and so on in succession. The blocks can be replaced in their original situations, by the application of the wheel battering-rams to the wedges, the power of which is so very great, that the weight of the ship can be taken from the shores that were placed to sustain her. There were one hundred and six ships of different classes, lifted at Plymouth dock-yard, from the 1st of January, 1798, to the 31st of December, 1800; and, had the operation of lifting taken less time, the number would have been very considerably increased; for the saving of a day is very frequently the cause of saving the spring tide, which makes the difference of a fortnight. The importance of this expedition, in time of war, cannot be sufficiently estimated.

This invention may be applied with great advantage, whenever it is necessary to erect shores, to support any great weights, as, for instance, to prop up a building during the repair of its foundation, &c. Captain Wells, of his Majesty's ship *Glory*, of 98 guns, used wedges of Mr. Seppings's invention for a fid of a top-gallant mast of that ship. In 1803, the top-gallant masts of the *Defence*, of 74 guns, were fitted on this principle by Mr. Seppings: and, from repeated trials, since she has been cruising in the North Sea, the wedge fids have been found in every respect to answer.

But it is Mr. Seppings's wish that it should be understood, that the idea of applying this invention to the fid of a top-gallant mast originated with Capt. Wells, who well understood the principle, and had received from him a model of the invention.

When it is required to strike a top-gallant mast, the top ropes are hove tight, and the pin which keeps the horizontal wedges in their place, is taken out, by one man going aloft for that purpose; the other horizontal wedge is worked in the

fid



fid, as shown in the drawing and model that accompany this statement. The upper part of the fid hole is cut to form the vertical wedge. The advantage derived from fidding top-gallant masts in this way is, that they can be struck at the shortest notice, and without slackening the rigging, which is frequently the cause of springing and carrying them away, particularly those with long pole heads. The angle of the horizontal wedges for the fids of masts should be about twenty degrees.

It can now be done without slackening the rigging.

The above Account was accompanied with Certificates from Sir John Henslow, Surveyor of the Navy; Mr. M. Didram, master-shipwright of Portsmouth-Yard; and Mr. John Carpenter, foreman of Sheerness Dock-yard, confirming Mr. Seppings's statement.

*Reference to the Engraving of Mr. Seppings's method of obviating the necessity of lifting Ships. Plate XI.*

This plan and section of a seventy-four gun ship describes the method of obviating the necessity of lifting ships, when there may be occasion to put additional false keels to them, or to make good the imperfections of those already on; also when it may be necessary to caulk the garboard seams, scarple the keel, &c. by which means a very considerable part of the expense will be saved, and much time gained. The blocks are cleared, and again returned by the following process. A sufficient number of shores are placed under the ship to sustain her weight, and set taught, stationed as near the keel as the working of the battering-rams fore and aft will admit. Avoid placing any opposite the blocks, as they would in that case hinder the return of the wedges with the battering-rams. A blow must then be given forward on the outer end of the iron wedges with the battering-rams in a fore and aft direction, which will cause them to slide aft, as shown in the plan. The battering rams abast then return the blow, and the wedges again come forward; by the repetition of this operation, the wedges will be with great ease cleared, and the angular block on the top will drop down. When the work is performed, the block must be replaced under the keel, and the wedges driven back by working the rams athwart-ships, as described in the section.

Description and reference to the plate, &c.

Instructions.

N. B. In



N. B. In returning the iron wedges, to avoid straining the angular blocks, it is proposed to leave a few of them out forward and aft, and stop the ship up, by laying one iron wedge on the other, as shown at Fig. 1, Plate XI.

To facilitate the business, blocks may be cleared forward and aft at the same time, sufficient to get in place one length of false keel. If the false keel should want repairing, it may be done without any additional shores, by clearing one block at a time, and when the keel is repaired in the wake of that block, return the wedges, as above directed, and clear the next, &c.

*Section and Plan, Plate XI. Fig. 2.*

Parts of the  
Section and plan.

- A. Keelson.
- B. Ceiling.
- C. Floor timber.
- D. Dead or rising wood.
- E. Plank of the bottom.
- F. Keel and false keel.
- G. Angular blocks with a half-inch iron-plate bolted to them.
- H. Cast-iron wedges.
- I. Iron plate of three-fourths of an inch thick on the bottom of the dock.
- K. Battering-rams, with wheels, and ropes for the hands.
- L. Cast-iron wedges, having received a blow from forward.
- M. Shores under the ship to sustain her weight.

*Fig. 3*, represents part of a top-gallant mast fitted with a wedge fid.

- a. Top-gallant mast.
- b. Fid, with one horizontal wedge worked on it.
- c. Moveable wedge, with the iron strap and pin over it, to keep it in its situation.
- d. Trussel trees.

## VII.

*On muscular Motion.* By ANTHONY CARLISLE, Esq. F. R. S.  
being the Croonian Lecture, read before the Royal Society,  
November 8, 1804.

ANIMAL physiology has derived several illustrations and additions, from the institution of this lecture on muscular motion; and the details of anatomical knowledge have been considerably augmented by descriptions of muscular parts before unknown. Introduction.

Still, however, many of the phenomena of muscles remain unexplained, nor is it to be expected that any sudden insulated discovery shall solve such a variety of complicated appearances.

Muscular motion is the first sensible operation of animal life: the various combinations of it sustain and carry on the multiplied functions of the largest animals: the temporary cessation of this motive faculty is the suspension of the living powers, its total quiescence is death. Muscular motion.

By the continuance of patient, well directed researches, it is reasonable to expect much important evidence on this subject, and, from the improved state of collateral branches of knowledge, together with the addition of new sources, and methods of investigation, it may not be unreasonable to hope for an ultimate solution of these phenomena, no less complete, and consistent, than that of any other desideratum in physical science.

The present attempt to forward such designs is limited to circumstances which are connected with muscular motion, considered as causes, or rather as a series of events, all of which contribute, more or less, as conveniencies, or essential requisites, to the phenomena; the details of muscular applications being distinct from the objects of this lecture.

No satisfactory explanation has yet been given of the state or changes which obtain in muscles during their contractions or relaxations, neither are their corresponding connections with the vascular, respiratory, and nervous systems, sufficiently traced. These subjects are therefore open for the present enquiry, and, although I may totally fail in this attempt to elucidate Neither the changes in muscles during action, nor their connections with the other parts of the system have been explained.

cidate any one of the subjects proposed, nevertheless I shall not esteem my labour useless, or the time of the Royal Society altogether unprofitably consumed, if I succeed in pointing out the way to the future attainment of knowledge so deeply interesting to mankind.

The muscle itself is fibrous.

The muscular parts of animals are most frequently composed of many substances, in addition to those which are purely muscular. In this gross state, they constitute a flexible, compressible solid, whose texture is generally fibrous, the fibres being compacted into fasciculi, or bundles of various thickness. These fibres are elastic during the contracted state of muscles after death, being capable of extension to more than one-fifth of their length, and of returning again to their former state of contraction.

The enveloping membrane is elastic.

This elasticity, however, appears to belong to the enveloping reticular or cellular membrane, and it may be safely assumed that the intrinsic matter of muscle is not elastic.

The attraction of cohesion, in the parts of muscle, is strongest in the direction of the fibres, it being double that of the contrary, or transverse direction.

Muscles are irritable during life.

When muscles are capable of reiterated contractions and relaxations, they are said to be alive, or to possess irritability. This quality fits the organ for its functions. Irritability will be considered, throughout the present lecture, as a quality only.

They have less cohesion lengthways when dead.

When muscles have ceased to be irritable, their cohesive attraction in the direction of their fibres is diminished, but it remains unaltered in the transverse direction.

Experiment in proof.

The hinder limbs of a frog attached to the pelvis being stripped of the skin, one of them was immersed in water at  $115^{\circ}$  of Fahrenheit, during two minutes, when it ceased to be irritable. The thigh bones were broken in the middle, without injuring the muscles, and a scale affixed to the angle of each limb: a tape passed between the thighs was employed to suspend the apparatus. Weights were gradually introduced into each scale, until, with five pounds avoirdupois, the dead thigh was ruptured across the fleshy bellies of its muscles.

repeated.

The irritable thigh sustained six pounds weight avoirdupois, and was ruptured in the same manner. This experiment was repeated on other frogs, where one limb had been killed by a watery solution of opium, and on another where essential oil

of



of cherry laurel <sup>was</sup> employed: in each experiment, the irritable limb sustained a weight one-sixth heavier than the dead limb.

It may be remarked, in confirmation of these experiments, <sup>The same doctrine confirmed by effects in the living subject.</sup> that when muscles act more powerfully, or more rapidly, than is equal to the strength of the sustaining parts, they do not usually rupture their fleshy fibres, but break their tendons, or even an intervening bone, as in the instances of ruptured tendo achillis, and fractured patella. Instances have however occurred, wherein the fleshy bellies of muscles have been lacerated by spasmodic actions; as in tetanus the recti abdominis have been torn asunder, and the gastrocnemii in cramps; but in those examples it seems that either the antagonists produce the effect, or the over-excited parts tear the less excited in the same muscle. From whence it may be inferred, that the attraction of cohesion in the matter of muscle is considerably greater during the act of contracting, than during the passive state of tone, or irritable quiescence, a fact which has been always assumed by anatomists from the determinate forces which muscles exert.

The muscular parts of different classes of animals vary in colour and texture, and not unfrequently those variations occur in the same individual. <sup>Differences observable in the colour, texture, &c. of muscular parts.</sup>

The muscles of fishes and vermes are often colourless, those of the mammalia and birds being always red: the amphibia, the accipenser, and squalus genera, have frequently both red and colourless muscles in the same animal.

Some birds, as the black game †, have the external pectoral muscles of a deep red colour, whilst the internal are pale.

In texture, the fasciculi vary in thickness, and the reticular membrane is in some parts coarse, and in others delicate: the heart is always compacted together by a delicate reticular membrane, and the external glutæi by a coarser species.

An example of the origin of muscle is presented in the history of the incubated egg, but whether the rudiments of the punctum saliens be part of the cicatricula organised by the parent, or a structure resulting from the first process of incubation, may be doubtful: the little evidence to be obtained on this point seems in favour of the former opinion; a regular

Origin of muscle in the egg.  
Punctum saliens

Distilled oil from the leaves of the *Prunus Laurecerassus*.

† *Tetrao tetrix*. Lin.

confirmation

confirmation of which would improve the knowledge of animal generation by shewing that it is gemmiferous. There are sufficient analogies of this kind in nature, if reasoning from analogies were proper for the present occasion.

The punctum saliens, during its first actions, is not encompassed by any fibres discoverable with microscopes, and the vascular system is not then evolved, the blood flowing forwards and backwards, in the same vessels. The commencement of life in animals of complex structure is, from the preceding fact, like the ultimate organization of the simpler classes.

Muscles of birds are formed out of the albumen, with a small portion of vitellus and atmosf. fluid,

It is obvious that the muscles of birds are formed out of the albumen ovi, the vitellus, and the atmospheric air, acted upon by a certain temperature. The albumen of a bird's egg is wholly consumed during incubation, and the vitellus little diminished, proving that the albumen contains the principal elementary materials of the animal thus generated; and it follows that the muscular parts, which constitute the greater proportion of such animals when hatched, are made out of the albumen, a small portion of the vitellus, and certain elements, or small quantities of the whole compound of the atmosphere.

and they do not differ from those of the mammalia.

The muscles of birds are not different, in any respect, from those of quadrupeds of the class of mammalia.

The anatomical structure of muscular fibres is generally complex, as those fibres are connected with membrane, blood-vessels, nerves, and lymphæducts; which seem to be only appendages of convenience to the essential matter of muscle.

Muscular fibre is cylindric; membranous without, and pulpy within.

A muscular fibre, duly prepared by washing away the adhering extraneous substances, and exposed to view in a powerful microscope, is undoubtedly a solid cylinder, the covering of which is reticular membrane, and the contained part a pulpy substance irregularly granulated, and of little cohesive power when dead.

The ultimate fibres.

A difficulty has often subsisted among anatomists concerning the ultimate fibres of muscles; and, because of their tenuity, some persons have considered them infinitely divisible, a position which may be contradicted at any time, by an hour's labour at the microscope.

Arteries.

The arteries arboresce copiously upon the reticular coat of the muscular fibre, and in warm-blooded animals these vessels are



are of sufficient capacity to admit the red particles of blood, but the intrinsic matter of muscle, contained within the ultimate cylinder, has no red particles.

The arteries of muscles anastomose with corresponding veins; but this course of a continuous canal cannot be supposed to act in a direct manner upon the matter of muscle.

The capillary arteries terminating in the muscular fibre must alone effect all the changes of increase in the bulk, or number, of fibres, in the replenishment of exhausted materials, and in the repair of injuries; some of these necessities may be supposed to be continually operating. It is well known, that the circulation of the blood is not essential to muscular action; so that the mode of distribution of the blood vessels, and the differences in their size, or number, as applied to muscles, can only be adaptations to some special convenience.

Another prevalent opinion among anatomists, is the infinite extension of vascularity, which is contradicted in a direct manner by comparative researches. The several parts of a quadruped are sensibly more or less vascular, and of different textures; and, admitting that the varied diameter of the blood vessels disposed in each species of substance, were to be constituted by the gross sensible differences of their larger vessels only, yet, if the ultimate vessels were in all cases equally numerous, then the sole remaining cause of dissimilarity would be in the compacting of the vessels. The vasa vasorum of the larger trunks furnish no reason, excepting that of a loose analogy, for the supposition of vasa vasorum extended without limits. Moreover, the circulating fluids of all animals are composed of water, which gives them fluidity, and of animalized particles of defined configuration and bulk; it follows that the vessels through which such fluids are to pass, must be of sufficient capacity for the size of the particles, and that smaller vessels could only filtrate water devoid of such animal particles: a position repugnant to all the known facts of the circulation of blood, and the animal economy.

Vascularity is not infinite, but perceptibly limited.

The capillary arteries which terminate in the muscular fibre, must be secretory vessels for depositing the muscular matter, the lymphæducts serving to remove the superfluous extravasated watery fluids, and the decayed substances which are unfit for use.



and lymphæ-  
ducts.

The lymphæducts are not so numerous as the blood vessels, and certainly do not extend to every muscular fibre: they appear to receive their contained fluids from the interstitial spaces formed by the reticular or cellular membrane, and not from the projecting open ends of tubes, as is generally represented. This mode of receiving fluids out of a cellular structure, and conveying them into cylindrical vessels, is exemplified in the corpora cavernosa, and corpus spongiosum penis, where arterial blood is poured into cellular or reticular cavities, and from thence it passes into common veins by the gradual coarctation of the cellular canals.

In the common green turtle, the lacteal vessels universally arise from the loose cellular membrane, situated between the internal spongy coat of the intestines and the muscular coat. The cellular structure may be filled from the lacteals, or the lacteals from the cellular cavities. When injecting the smaller branches of the lymphæducts retrograde in an œdematous human leg, I saw, very distinctly, three orifices of these vessels terminating in the angles of the cells, into which the quicksilver trickled. The preparation is preserved, and a drawing of the appearance made at the time. It was also proved, by many experiments, that neither the lymphæducts, nor the veins, have any valves in their minute branches.

Nerves of volun-  
tary muscles.

The nerves of voluntary muscles separate from the same bundles of fibrils with the nerves which are distributed in the skin, and other parts, for sensation; but a greater proportion of nerve is appropriated to the voluntary muscles, than to any other substances, the organs of the senses excepted.

Origin of the  
nerves of voli-  
tion.

The nerves of volition all arise from the parts formed by the junction of the two great masses of the brain, called the Cerebrum and Cerebellum, and from the extension of that substance throughout the canal of the vertebræ. Another class of muscles, which are not subject to the will, are supplied by peculiar nerves; they are much smaller, in proportion to the bulk of the parts on which they are distributed, than those of the voluntary muscles; they contain less of the white opaque medullary substance than the other nerves, and unite their fibrils, forming numerous anastomoses with all the other nerves of the body, excepting those appropriated to the organs of the senses. There are enlargements at several of these

Muscles for in-  
voluntary  
motion.

junctions.

junctions, called Ganglions, and which are composed of a less proportion of the medullary substance, and their texture is firmer than that of ordinary nerves.

The terminal extremities of nerves have been usually considered of unlimited extension; by accurate dissection however, and the aid of magnifying glasses, the extreme fibrils of nerves are easily traced as far as their sensible properties, and their continuity extends. The fibrils cease to be subdivided whilst perfectly visible to the naked eye, in the voluntary muscles of large animals, and the spaces they occupy upon superficies where they seem to end, leave a remarkable excess of parts unoccupied by those fibrils. The extreme fibrils of nerves lose their opacity, the medullary substance appears soft and transparent, the enveloping membrane becomes pellucid, and the whole fibril is destitute of the tenacity necessary to preserve its own distinctness; it seems to be diffused and mingled with the substances in which it ends. Thus the ultimate terminations of nerves for volition, and ordinary sensation, appear to be in the reticular membrane, the common covering of all the different substances in an animal body, and the connecting medium of all dissimilar parts.

By this simple disposition, the medullary substance of nerve is spread through all organized, sensible, or motive parts, forming a continuity which is probably the occasion of sympathy. Peculiar nerves, such as the first and second pairs, and the portio mollis of the seventh, terminate in an expanse of medullary substance which combines with other parts and membranes, still keeping the sensible excess of the peculiar medullary matter.

The peculiar substance of nerves must in time become insufficient; and, as it is liable to injuries, the powers of restoration, and repair, are extended to that material. The re-union of nerves after their division, and the reproduction after part of a nerve has been cut away, have been established by decisive experiments. Whether there is any new medullary substance employed to fill up the break, and, if so, whether the new substance be generated at the part, or protruded along the nervous theca from the brain, are points undetermined: the history of the formation of a foetus, the structure of certain monsters, and the organization of simple animals, all seem to

The extremities of nerves are definite in their termination.

Diffusion of nervous matter.

Restoration or repair of nerves.



favour the probability, that the medullary matter of nerves is formed at the parts where it is required, and not in the principal seat of the cerebral medulla.

Whether the matter of nerve be not extensively mixed in all irritable parts.

This doctrine, clearly established, would lead to the belief of a very extended commixture of this peculiar matter in all the sensible and irritable parts of animals, leaving the nerves in their limited distribution, the simple office of conveying impressions from the two sentient masses with which their extremities are connected. The most simple animals in whom no visible appearances of brain or nerves are to be found, and no fibrous arrangement of muscles, may be considered of this description: Mr. John Hunter appeared to have had some incomplete notions upon this subject, which may be gathered from his representation of a *materia vitæ* in his *Treatise on the Blood*, &c. Perhaps it would be more proper to distinguish the peculiar matter of muscle by some specific term, such, for example, as *materia contractilis*.

Peculiar adaptation for the nerves of electric animals.

A particular adaptation for the nerves which supply the electrical batteries of the torpedo, and gymnotus, is observable on the exit of each from the skull; over which there is a firm cartilage acting as a yoke, with a muscle affixed to it, for the obvious purpose of compression: so that a voluntary muscle probably governs the operations of the battery.

The matter of the nerves, and brain, is very similar in all the different classes of animals.

The external configuration of animals is not more varied than their internal structure.

Configuration and structure of the various classes of animals.

The bulk of an animal, the limitation of its existence, the medium in which it lives, and the habits it is destined to pursue, are each, and all of them, so many indications of the complexity or simplicity of their internal structure. It is notorious that the number of organs, and of members, is varied in all the different classes of animals; the vascular and nervous systems, the respiratory, and digestive organs, the parts for procreation, and the instruments of motion, are severally varied, and adapted to the condition of the species. This modification of anatomical structure is extended in the lowest tribes of animals, until the body appears to be one homogeneous substance. The cavity for receiving the food is indifferently the internal, or external surface, for they may be inverted, and still con-

Very simple animals.

continue



time to digest food; the limbs or tentacula may be cut off, and they will be regenerated without apparent inconvenience to the individual: the whole animal is equally sensible, equally irritable, equally alive: its procreation is gemmiferous. Every part is pervaded by the nutritious juices, every part is acted upon by the respiratory influence, every part is equally capable of motion, and of altering its figure in all directions, whilst neither blood-vessels, nerves, nor muscular fibres, are discoverable by any of the modes of investigation hitherto instituted.

From this abstract animal (if such a term may be admitted) up to the human frame, the variety of accessory parts, and of organs by which a complicated machinery is operated, exhibit infinite marks of design, and of accommodations to the purposes which fix the order of nature.

In the more complicated animals, there are parts adapted for trivial conveniences, much of their materials not being alive, and the entire offices of some liable to be dispensed with. The water transfused throughout the interstitial spaces of the animal fabric, the combinations with lime in bones, shells, and teeth; the horns, hoofs, spines, hairs, feathers, and cuticular coverings, are all of them, or the principal parts of their substance, extra-vascular, insensible, and unalterable by the animal functions after they are completed. I have formed an opinion, grounded on extensive observation, that many more parts of animal bodies may be considered as inanimate substances; even the reticular membrane itself seems to be of this class, and tendons, which may be the condensed state of it; but these particulars are foreign to the present occasion.

The deduction now to be made, and applied to the history of muscular motion, is, that animated matter may be connected with inanimate; this is exemplified in the adhesions of the muscles of multi-valve, and bi-valve shell fish, to the inorganic shell, the cancer Bernhardus to the dead shells of other animals, and in the transplantation of teeth. All of which, although somewhat contrary to received opinion, have certainly no degree of vascularity, or vital connection with the inhabitant; these shells being liable to transudations of cupreous salts and other poisonous substances, whilst the animal remains uninjured. A variety of proofs to the same

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effect might be adduced, but it would be disrespectful to to this learned body to urge any farther illustrations on a subject so obvious.

Division of the parts of an animal destroys the conformation;

The effects of subdivision, or comminution of parts among the complicated organized bodies, is unlike that of mineral bodies: in the latter instance, the entire properties of the substance are retained, however extensive the subdivision; in the former substances, the comminution of parts destroys the essential texture and composition, by separating the gross arrangements of structure upon which their specific properties depend. From similar causes it seems to arise, that animals of minute bulk are necessarily of simple structure: size alone is not, however, the sole cause of their simple organization, because examples are sufficiently numerous wherein the animal attains considerable bulk, and is of simple structure, and *vice versa*: but, in the former, the medium in which they live, and the habits they assume, are such as do not require extensive appendages, whilst the smaller complex animals are destined to more difficult, and more active exertions. It may be assumed however, as an invariable position, that the minutest animals are all of simple organization.

but less the more simple the structure.

Life may on a small scale be supported with simple materials; bulky animals require variety of organs.

Upon a small scale, life may be carried on with simple materials; but the management, and provisions for bulky animals, with numerous limbs, and variety of organs, and appendages of convenience, are not effected by simple apparatus; thus, the skeleton which gives a determinate figure to the species, supports its soft parts, and admits of a geometrical motion, is placed interiorly, where the bulk of the animal admits of the bones being sufficiently strong, and yet light enough for the moving powers; but the skeleton is placed externally, where the body is reduced below a certain magnitude, or where the movements of the animal are not to be of the floating kind: in which last case the bulk is not an absolute cause. The examples of testaceous vermes, and coleopterous, as well as most other insects, are universally known.

Thus large animals have their bones within them, smaller have them without.

Crystalline of the eye, muscular.

The opinion of the muscularity of the crystalline lens of the eye, so ingeniously urged by a learned member of this Society, is probably well founded; as the arrangement of radiating lines of the matter of muscle, from the centre to the circumference of the lens, and these compacted into angular masses, would produce specific alterations in its figure.

This



This rapid sketch of the history of muscular structure has been obtruded before the Royal Society to introduce the principal experiments, and reasonings which are to follow: they are not ordered with so much exactness as becomes a more deliberate essay, but the intention already stated, and the limits of a lecture are offered as the apology.

Temperature has an essential influence over the actions of muscles, but it is not necessary that the same temperature should subsist in all muscles during their actions; neither is it essential that all the muscular parts of the same animal should be of uniform temperatures for the due performance of the motive functions. Temperature of muscles.

It appears that all the classes of animals are endowed with some power of producing thermometrical heat, since it has been so established in the amphibia, pices, vermes, and insecta, by Mr. John Hunter; a fact which has been verified to my own experience; the term "cold-blooded" is therefore only relative. The ratio of this power is not, however, in these examples, sufficient to preserve their equable temperature in cold climates, so that they yield to the changes of the atmosphere, or the medium in which they reside, and most of them become torpid, approaching to the degree of freezing water. Even the mammalia, and aves, possess only a power of resisting certain limited degrees of cold; and their surfaces, as well as their limbs, being distant from the heart, and principal blood-vessels, the muscular parts so situated are subject to considerable variations in their temperature, the influence of which is known. All animals do produce heat;

In those classes of animals which have little power of generating heat, there are remarkable differences in the structure of their lungs, and in the composition of their blood, from the mammalia and aves. The colder animals.

Respiration is one of the known causes which influences the temperatures of animals: where these organs are extensive, the respirations are performed at regular intervals, and are not governed by the will, the whole mass of blood being exposed to the atmosphere in each circulation. In all such animals living without the tropics, their temperature ranges above the ordinary heat of the atmosphere, their blood contains more of the red particles than in the other classes, and their muscular irritability ceases more rapidly after violent death. Respiration: its organs in animals of higher temperature;

The



and in the cold-blooded :

The respirations of the animals denominated "cold-blooded," are effected differently from those of high temperature ; in some of them, as the amphibia of Linnæus, the lungs receive atmospheric air, which is arbitrarily retained in large cells, and not alternately, and frequently changed. The fishes, and the testaceous vermes, have lungs which expose their blood to water, but whether the water alone, or the atmospheric air mingled with it, furnish the changes in the pulmonary blood, is not known.

in insects.

In most of the genera of insects, the lungs are obrescent tubes containing air, which, by these channels, is carried to every vascular part of the body. Some of the vermes of the simpler construction have no appearance of distinct organs, but the respiratory influence is nevertheless essential to their existence, and it seems to be effected on the surface of the whole body.

In all the colder animals, the blood contains a smaller proportion of the red colouring particles than in the mammalia, and aves ; the red blood is limited to certain portions of the body, and many animals have none of the red particles.

Experiment :  
Cold-blooded animals were included in water over mercury. After some days they died ; but no gas was emitted, nor was the water changed.

The following animals were put into separate glass vessels, each filled with a pound weight of distilled water, previously boiled to expel the air, and the vessels inverted into quicksilver ; viz. one gold fish, one frog, two leeches, and one fresh-water muscle.\* These animals were confined for several days, and exposed in the sun in the day-time, during the month of January, the temperature being from 43° to 48°, but no air bubbles were produced in the vessels, nor any sensible diminution of the water. The frog died on the third day, the fish on the fifth, the leeches on the eighth, and the fresh-water muscle on the thirteenth. This unsuccessful experiment was made with the hope of ascertaining the changes produced in water by the respiration of aquatic animals, but the water had not undergone any chemical alteration.

Hybernating animals can live under confined respiration.

Peculiarity of structure in the heart and its veins.

Animals of the class mammalia which hybernate, and become torpid in the winter, have at all times a power of subsisting under a confined respiration, which would destroy other animals not having this peculiar habit. In all the hybernating mammalia there is a peculiar structure of the heart, and its principal veins ; the superior cava divides into two trunks ; the left, passing over the left auricle of the heart, opens into

\* *Mytilus Anatinus.*

the inferior part of the right auricle; near to the entrance of the vena cava inferior. The veins usually called azygos, accumulate into two trunks, which open each into the branch of the vena cava superior, on its own side of the thorax. The intercostal arteries and veins in these animals are unusually large.

This tribe of quadrupeds have the habit of rolling up their bodies into the form of a ball during ordinary sleep, and they invariably assume the same attitude when in the torpid state: the limbs are all folded into the hollow made by the bending of the body; the clavicles, or first ribs, and the sternum, are pressed against the fore part of the neck, so as to interrupt the flow of blood which supplies the head, and to compress the trachea: the abdominal viscera, and the hinder limbs are pushed against the diaphragm, so as to interrupt its motions, and to impede the flow of blood through the large vessels which penetrate it, and the longitudinal extension of the cavity of the thorax is entirely obstructed. Thus a confined circulation of the blood is carried on through the heart, probably adapted to the last weak actions of life; and to its gradual recommencement.

Their habit and circumstances that accompany the torpid state, &c.

This diminished respiration is the first step into the state of torpidity; a deep sleep accompanies it; respiration then ceases altogether; the animal temperature is totally destroyed, coldness and insensibility take place, and finally the heart concludes its motions; and the muscles cease to be irritable. It is worthy of remark that a confined air, and a confined respiration, ever precede these phenomena: the animal retires from the open atmosphere, his mouth and nostrils are brought into contact with his chest, and enveloped in fur; the limbs become rigid, but the blood never coagulates during the dormant state. On being roused, the animal yawns, the respirations are fluttering, the heart acts slowly and irregularly, he begins to stretch out his limbs, and proceeds in quest of food. During this dormancy, the animal may be frozen, without the destruction of the muscular irritability, and this always happens to the garden snail\*, and to the chrysalides of many insects during the winter of this climate.

The manner in which that state commences and is afterwards terminated.

(The conclusion in our next.)

\* *Helix nemoralis*.

Description

## VIII.

*Description of a Boring Tube, in general Use in America; but less known in this Country.*

Description of  
the American  
borer.

FIG. 2. Plate X. exhibits a very simple and ingenious borer, consisting of the common center bit of the carpenters followed by a wide flat thread screw, hammered up from a plate of iron or steel. It is said that they are used to bore holes several feet in length, and the peculiar property possessed by this instrument is, that it clears the cutting without requiring to be drawn out, as is the case with the augur, the gimblet, and other similar tools. I do not, however, think that it would have this effect in boring perpendicularly down to considerable depths; but for horizontal or slightly inclined holes, its effect must fully answer.

Popular explanation of its  
action.

It may not at first occur to the reader why the introduction of this tool into a hole which must contain the wood that formerly blocked it up, should not be attended with some degree of impediment or jamming; but this difficulty will vanish, when it is considered that the cuttings are, partly by their weight, and partly by friction against the internal cylindrical surface, prevented from revolving along with the screw. The consequence is that they are pressed against its thread, and slide along it towards the handle. And as this motion or shifting of the thread is quicker than the motion of boring, by which the whole tool is carried inwards, the cuttings must come out with a velocity nearly equal to the difference of these two motions.

## IX.

*Geographical and Topographical Improvements. By JOHN CHURCHMAN, Esq. M. Imp. Acad. of Sciences, Peterburgh.\**

Great value of  
topographical  
knowledge.

IT appears to be a matter of much importance to the people of any country, at all times, whether in war or peace, to possess

\* From the Transf. of the Soc. of Arts for 1804, who voted the silver medal to him for the same.



sefs a complete knowledge of its surface. In war, such knowledge is absolutely necessary for defence; in peace, for improving the country to the best advantage.

Now, since geography may be improved, an easy and accurate method to lay down maps of mountainous countries and hilly estates, will perhaps prove useful, as it will show at a single view the true shape and comparative height of the ground without the art of painting.

Utility of a correct delineation of hills, &c.

As mountains are apt to eclipse each other, a perspective view is seldom very extensive, the rules of which fall short of giving an accurate idea of any hilly country; because such a view, though strictly true in one particular place, is not so in any other. The altitudes of mountains appear in proportion to the distance from the eye, and no rule in geometry has been found sufficient to determine distances from any single station. Neither can a bird's-eye view of an estate ascertain the depth of valleys or the height of mountains. But the method here proposed will be found equally capable of giving the true shape of any ground above or below water. It may be successfully applied to sea charts, and will prevent much confusion, arising from the tedious method of distinguishing soundings by a multitude of figures.

Mountains cannot be usefully shewn in perspective.

### *Explanation.*

Suppose a full description is required of any island in the ocean. First, let an accurate map be laid down in the common way; and let the perpendicular height between the highest point of land and the ocean be divided into any number of equal parts. Suppose these equal divisions are 100, 200, 300, 400 feet above the low-water mark. From the different points of these several divisions, let horizontal lines be run with a good theodolite, and spirit level annexed, all round the island. If the work is well done, each line will end where it began; and if the bearings and distances of these several lines are truly laid down on the map, the crooked courses of them will clearly show the shape of the ground over which they pass. For example: if any horizontal line passes by the side of a steep hill, it will incline towards the ocean, or approach the next horizontal line below it. When the same line crosses a stream of running water or a valley, it will naturally bend up the side of the said stream, until it can cross it without losing the

New method.  
Make a good map or plan.  
Mark the point of highest elevation and other points differing in elevation by equal measures.  
Run lines with the theodolite and level from these points; in which if the survey be good, they will again terminate.  
These lines laid on the map will shew the figure of the country.

the level; or, in other words, it will bend towards the centre of the island. Hence, by a little practice, the shape of the several horizontal lines on the map will give as clear an idea to the mind, of the shape of any country over which they pass, as a sight of the country itself can convey to the eye. But to obtain a mathematical and true knowledge of the altitude and declivity of any part of the country, we have the following proposition:

Trigonometrical  
rule for the de-  
clivities.

As the perpendicular height of any one horizontal line above another is to the radius: so is the horizontal distance between the horizontal lines measured on the map at any particular place: to the co-tangent of declivity at that place.

*Note.*---If the horizontal distance between any two horizontal lines on the map is equal to the perpendicular height of any horizontal line above another, the angle of altitude, or declivity, of any hill will be 45 degrees.

Advantages;

The present improvement, which I believe to be entirely new, will be found to possess the following advantages:

—to military  
men,

1st. Military men are well acquainted with the many advantages always to be gained from the exact representation of high grounds. By this method, we are able to give the angle of altitude, the angle of declivity, and perpendicular height of every hill; likewise the comparative height of different hills, the best route by which the high grounds may be gradually ascended, and where heavy burthens can be drawn up with most ease.

—and for domes-  
tic and econo-  
mical purposes.

2dly. Experience has sufficiently shown, that the inhabitants of low grounds are subject to different kinds of sickness, from which those living at places elevated to a certain degree are exempt. A map on this improved plan will point out the most proper situation for building dwelling-houses. It will be useful in botany, in discovering or cultivating some kinds of plants which flourish best at particular distances above the level of the ocean. It will trace the line of vegetation on the sides of lofty mountains, whose tops are covered with eternal snow.

—to direct agri-  
cultural improve-  
ments,

3dly. Some high lands are known to produce good grain, while low lands afford grass more abundantly; but most grounds produce good grass, over which a moderate quantity of running water is conveyed. A plan of any country in this way will show all the ground that can be irrigated; where water-



water-works may be erected; where navigable canals may be cut; and where high-ways and rail-roads may be laid out on the best and most level ground.

4thly. The subterraneous treasures of the mineral and fossil kingdoms are generally found in strata; and if they are not truly horizontal, they make a certain angle with the horizon. A map on this projection may enable the mineralogist to follow any one stratum, at places even far distant from each other. —and mine researches.

#### Problem.

To find the true declivity of any piece of ground, in any map laid down on the principles of the present plan. Examples computed.

#### Example 1st. for D. see Plate IV.

As the perpendicular height, 4 feet	-	60206
Is to radius, $90^{\circ}$	-	10.00000
So is the horizontal distance, 4 feet	-	60206
		<hr/>
		10.60206
To the co-tangent of the declivity, $45^{\circ}$	-	10.00000

#### Example 2d. for B.

As the perpendicular height, 4 feet	-	60206
Is to radius, $90^{\circ}$	-	10.00000
So is the horizontal distance, 8 feet	-	90309
		<hr/>
		10.90309
To the co-tangent of the declivity, $26^{\circ} 34'$	-	10.30103

#### Example 3d. for C.

As the perpendicular height, 4 feet	-	60206
Is to radius, $90^{\circ}$	-	10.00000
So is the horizontal distance, 18 feet	-	1.25527
		<hr/>
		11.25527
To the co-tangent of the declivity, $12^{\circ} 32'$	-	10.65321

The annexed survey, Plate IX. of a small lake and artificial mountain in the garden of his Excellency Count de Strogonoff, near St. Petersburg, has been closed by the tables of the difference of latitude and departure, as follows: A survey according to this method.

N 30 E.



		N	S	E	W
N 30 E	2½	2.2	—	1.2	—
N 35 E	2	1.6	—	1.1	—
N 75 E	2	.5	—	1.9	—
N 55 E	2	1.1	—	1.6	—
N 45 E	3	2.1	—	2.1	—
N 52 W	2	1.2	—	—	1.6
N 59 W	3	1.5	—	—	2.5
S 56 W	12	—	6.7	—	9.9
S 60 E	7	—	3.5	6.1	—
		10.2	10.2	14.0	14.0

## X.

*A Memoir on the refining of Lead in the large Way. Containing some Reflections on the Inconveniencies resulting from Cupels made of Ashes; with a new and æconomical Method of constructing those Cupels. By CITIZEN DUHAMEL.\**

Process for re-  
fining lead.  
Cupels made of  
ashes.

EVERY one knows that in order to effect the separation of silver from lead, a process called refining or cupellation is made use of, which is effected in a basin called a cupel; and it is likewise known that this basin is formed of the ashes or incinerated remains of animal or vegetable substances, after depriving them by washing of what saline matter they may contain.

The great quantity of wood ashes required for making these cupels, and the difficulty of procuring it, have long ago induced me to seek for a more simple and less expensive means of forming these vessels.

The early chemists having observed that lead becomes oxidized and converted into what is called litharge, when it is ex-

\* This memoir is translated from the Memoirs of the French National Institute, Vol. III. at the request of a correspondent.

Early observa-  
tion that the  
oxide of lead  
penetrates che-  
mical vessels,  
while silver, if  
present, remains  
metallic.

posed to fire with the contact of atmospheric air, while the silver it may contain, preserves its metallic form; it only remained for them to contrive a method of separating these two metals. They were led to this method by observing that the oxide of lead, in its state of liquefaction easily penetrates the substances with which it may be in contact, particular bone ashes, without destroying the figure of the vessel made of that material. In fact there is no substance whatever which is better adapted to form the small cupels for assaying.

The difficulty, and often the impossibility of procuring three or four bushels of bone ashes in Germany, for each time of refining, has led to the adoption of the ashes of wood. But not to mention that these are of considerable price, and not always to be had, they present another inconvenience, by often rising and floating on the surface of the lead. When this happens, the process must fail; and it does happen as often as the ashes are ill prepared, or the cupel not sufficiently or irregularly beaten, or that the openings left for the evaporation of the humidity are not properly disposed, or enough in number, or closed by a portion of the scoriæ upon which the floor is made to receive the ashes. This floor ought to be constructed of the most porous bricks, in order that the water with which the ashes must be wetted may penetrate them and evaporate into the bed of scoria, and escape through the opening at the bottom of the furnace.

The elasticity of this aqueous vapour frequently causes explosions, which not only disorder the cupel, but even the masonry of the furnace, if it be not properly constructed.

In order to ascertain the proportion of silver in any quantity of lead, it is only necessary to pass a few pennyweights into a small cupel of bone ashes placed under the muffle of an assayer's furnace; as the lead becomes oxidized it is imbibed in the cupel, and at length the phenomenon of brightening takes place upon the metallic button. This appearance shews that all the lead is dissipated, and the remaining silver in a pure state.

In the large way of refining, the same object of separating the silver from the lead is aimed at, but the lead is not intended to penetrate into the cupel, which in fact is impracticable. For the total absorption of this metal would require a much greater quantity of ashes, with the consumption of a ten

For want of bone ashes the ashes of wood are used for cupels; they have many inconveniences.

Cupelling of lead.

Refining in the large way cannot be done by the assaying process wherein the oxide is absorbed.

fold portion of time and fuel; besides which, the loss in recovering the lead by fusion of the cupel is very expensive and the product less considerable than in the common way. The oxide of lead obtained in this last method may be easily fused, and reduced if needful; but it is an article of value in the arts, and therefore very acceptable in the market in its state of oxide.

Vessels of clay used in smelting lead.

The lead ores and litharge may be fused as is done in England and Britanny, in a reverberatory furnace, of which the floor or basin is formed of moistened and rammed clay. These floors resist the action of fire as well as that of the oxide of lead during six or eight months constant work.

These are applicable to refining.

The durability of these basins of earth first gave me a notion of the method I shall propose for refining furnaces, in which the intention is to oxide the lead in order to obtain litharge, and not to cause it to be totally absorbed in the cupels, as is done when the metal is assayed, to shew how much silver it contains.

It would be preferable that no absorption took place.

In the operation upon a large scale the cupel, though of ashes absorbs only part of the lead, as I have already remarked, observing at the same time that it would be much more advantageous to obtain the whole converted into litharge, of which the reduction into lead is infinitely more easy than that of the oxide contained in the ashes, which resist fusion, and afford a scoria that always contains some metal.

English process. 12 tons of lead are oxidized with scarcely any absorption.

Upon a cupel of ashes rammed into an oval ring of iron, about five feet in length and three and a half in width, the English refine in succession about twelve ton of lead, which becomes converted into fine merchantable litharge, with the exception of the small portion that penetrates the cupel, of which the thickness is less than three inches. This cupel is supported under the roof of the furnace by two bars of iron. The litharge is driven by the blast of bellows towards the anterior part of the furnace, whence it falls without interruption upon the area of the foundery; and at the same time to supply the space which this subtraction of oxide would leave, a pig of lead is gradually advanced into the interior of the furnace, placed on one side of the nozel of the bellows. This lead, by its gradual fusion, keeps the cupel full till towards the end of the operation.



I have given this sketch of the English process only to shew The vessel is very cheap. that it is possible to refine lead with very little expence of ashes for forming the cupels. Those here mentioned do not absorb eighty pounds of oxide out of the large quantity of lead thus refined.

Hence we see that metallurgists have always endeavoured to obtain the greatest possible quantity of litharge and the least of ashes containing oxide; but as they did not imagine they could depart from the docimaistic process, they have constantly made their cupels of ashes.

We have seen that in the small process of cupellation the lead penetrates the ashes as it becomes oxidized, and that when The litharge runs off in the small process; no more lead remains the small button of silver remains pure at the bottom in the spherical form. This operation is effected with more speed because the surface of the bath is always convex in these small vessels, and consequently the litharge runs off on all sides towards the edge of the cupel, where it is immediately imbibed.

This is not the case in large cupels of several yards diameter. Bellows must be used, not only to accelerate the oxidation by their blast, but to drive the litharge towards the passage or gutter which is left for its issue. —but cannot in the large.

The inconveniencies and even the impossibility of causing It must be driven off by bellows. all the lead to penetrate the ashes of these large cupels have been already remarked. This must be evident on reflecting that the oxidation can take place only at those parts of the bath which are exposed to the contact of the air and the blast of the bellows. The litharge near the middle of the basin not being disposed to flow towards the edge, would cover and defend the metal from any farther oxidation. Hence it is that the operators have found themselves obliged to drive out the litharge by the mechanical action of a stream of air from bellows.

The oxidation therefore takes place only at the surface of The oxidation is effected only at the surface. the lead, and not lower; if it were otherwise, the ashes of the cupel would be penetrated with oxide to a depth which would be more unequal the longer the operation lasted. Now I have always remarked that the portion of ashes thus imbibed in the large refinery is not thicker towards the center of the basin than towards its edge, though the lead remains thirty or forty times longer at the bottom than near the edges, because the

bath constantly diminishes till all the lead is reduced into litharge, and nothing at last remains but the button of silver at the bottom of the cupel.

Other reasons why the large process cannot be made by absorption.

That the whole of the lead is absorbed in the cupel of assay, arises from the unequal action of heat upon every part of the small vessel. As the cupel in the large way presents only its superior surface to the action of the heat, the oxide thus imbibed ceases to penetrate at the place where the temperature is no longer in a state to hold the oxidizing fusion. For this reason it is that the whole of the cupel is throughout impregnated to an equal depth, and it is impossible to cause all the lead to penetrate the ashes.

Litharge is sought as a product in the large way.

From the preceding observations it will be easy to conclude that though the assay of lead must be made in small cupels of bone ashes, in order that the whole of the oxidized metal may either be absorbed or partly evaporated; yet the case is very different in the large operation, where the object is to proceed with celerity, and to obtain as much litharge as possible.

Addition of sand to the wood ashes, &c.

I have before stated that the wood ashes used in forming large cupels are expensive, and frequently not to be procured in sufficient quantity; to which I have added their being subject to blow up or rise entirely, which occasions a considerable loss. It must further be mentioned, that in order to give more weight and consistence to these cupels it is often necessary to mix a considerable quantity of sand with them, particularly if the lead should contain foreign substances, such as arsenic, cobalt, antimony, zinc, tin and other matters. If the lead be merely arsenical, after having taken off the first scum, it is usual to throw from time to time, on the whole surface of the bath, about 20lbs. of iron scales or granulated crude iron. This iron being lighter than the lead, floats on the top and absorbs the arsenic, after which it is cleared away, and then the litharge is formed without any obstacle. This method is used in Saxony.

The necessity of adding sand to the ashes of the cupels ought to have led to a discovery which I propose: it is as follows.

#### *New Construction of the Cupels or Basons for refining Lead.*

New construction of the basons or vessels for refining lead.

Without making any change in the masonry of the furnaces for refining by what we call the German method, it is only necessary to be careful to make a sufficient number of vents in their

their base for the evaporation of the moisture, and to dispose of them so as most effectually to answer this purpose. These channels or vents are to be covered with a bed of scoria, upon which a pavement is to be made of the most porous bricks, and of the thickness of a single brick.

On this area or pavement, which ought to be concave like the base upon which the ashes of the ordinary cupels are placed, must be laid a quantity of foundry's sand, a little moistened. If it be not adhesive enough, a little clay may be added, in order to give the requisite solidity, and the whole carefully mixed. The sand must be rammed down in the same manner as is done to consolidate the ashes in the usual way, and a basin for refining must be formed, equally rammed in all its parts. The thickness of this cupel should be about six inches; and it may be made in two layers, as we shall hereafter observe.

The cupel is made of sand with a little clay instead of bone ashes.

After the basin has been in all parts uniformly beaten or rammed down, it will be proper to sift over the whole surface two or three quarts of wood ashes, which may be made to adhere by ramming.

When the cupel is thus prepared, the head of the furnace must be lowered, and a moderate fire kept up for several hours, in order to evaporate part of the water from the sand. The rest will be driven out, without inconvenience, through the vents during the refining.

It may be dried before use;

After a sufficient drying, which may even be dispensed with; the head is to be raised, the cupel, suffered to cool a little, and straw or hay then laid upon it, and upon this the pigs or pieces of lead, which are to be gently put down, in order that their weight may not make impressions in the sand. The straw is used for this purpose, in our method as well as the common method; and it would be convenient that the lead should be cast in iron hemispherical moulds or pots instead of the prismatic form, as these pieces would be less subject to damage the cupel.

—but this is not absolutely necessary.

Method of charging with lead.

When the quantity of lead necessary to fill the cupel is arranged in the furnace, the head is to be lowered and luted all round with clay, after which the fire is to be applied as in the usual processes.

When the lead is in perfect fusion, and the bath covered with dross and coaly matter from the straw, this skum must be

Fusion. Clearing of the surface.



raked off through the passage left for the litharge, by means of a wooden rake about a foot long, with an iron handle of sufficient length to reach every part of the bath.

Method of  
blowing, &c.

When the lead has been several times skimmed, and begins to become red, the bellows must be set into action, gently at first, and afterwards more strongly. Their nozles must be so disposed that the blast may be directed towards the center of the bath, and in order that the wind may be urged upon the surface of the metal, each nozle must have a small round plate of iron adapted to it. These small flaps or valves, called papillons (flies or butterflies) are used in the German fineries. They have an hinge at top, and at every stroke they rise about half way from their perpendicular position towards the level, so that by reflecting the air downwards upon the lead, they hasten its oxidation.

Gutter for the  
litharge.

When all the dross has been removed, and the lead is of a good red heat and covered with litharge, a little gutter must be made, with an hook appropriated for this purpose, in the sand of the cupel. This must be carefully done until the bottom of the gutter answers to the level of the bath. The litharge driven by the blast of the bellows will flow out of this passage, and fall upon the hearth of the foundery.

Instructions.

When the operator perceives that only a small quantity of litharge remains near the gutter, he will stop its escape with a small quantity of moistened ashes; but as soon as the lead shall be again covered with oxide, the passage must be opened and made deeper as the quantity of matter becomes depressed; taking care that no lead escapes, particularly towards the end of the operation, as it would carry along with it a large portion of silver which would be lost.

In this manner the process is to be carried on until the surface of the silver exhibits those flashes which are called the brightening, taking care to raise the fire in proportion to the diminution of the bath, particularly towards the end, when the silver is collected; and as this metal is much more difficult to be kept in fusion than its small alloy of lead, the refining will be imperfect, unless the temperature be raised, and instead of about one twentieth of lead, which the silver usually retains in the German method, it will remain much more highly charged. This would render it more difficult to be treated in the second operation, called the silver refining, or by the Germans *silver brenen*, by which it is rendered pure.

Those who are accustomed to refine lead in the German method, will find no difficulty in following mine. For though the cupel is made of sand instead of ashes, there is no difference in the manipulations.

We have seen that the English refine a great quantity of lead on a small cupel. The same may be done in the method here described, by adding in proportion as the loss by oxidation takes place. Supposing the capacity of the cupel to be such as to contain about five ton of lead, we might continue the operation to three times that quantity in a single process, which would not have the inconveniences of the English method.

I flatter myself that a well-made cupel of sand may be used for several refinings, without requiring to be made up again every time like those of ashes; but in these circumstances, and before the lead is put in, it is necessary to fill with well beaten sand the gutter or opening which was before made for carrying off the litharge. Care must be taken in doing this to remove with a chissel that kind of glaze which the oxide of the lead leaves behind it. With this precaution, after wetting the part where the new moistened sand is to be applied, they will firmly unite together.

From the long duration of the earthen floors of those reverberatory furnaces in which lead ores and even litharge are fused, as was before mentioned, we have no reason to apprehend any bad consequences from the oxide of lead, which acts only on the surface of the cupel, and penetrates to an inconsiderable depth.

After one or two refinings, this crust of oxide may be taken off and fused in a blast furnace, in order to recover the lead; a process no less easy than that of reducing the metal which exists in much greater quantity in the ordinary cupels. We therefore obtain a larger quantity of litharge, which is one advantage, and in addition to this, the loss in silver which accompanies the absorbed lead will be less. For in the small quantity of precious metal which accompanies the oxide, it is found by experiment, that the proportion is greater in the absorbed lead than in that which is driven over in litharge.

Instead of sand we might make use of clay in constructing our cupels, as is done in the hearths of the reverberatory furnaces of Brittany; but it would then be necessary to pound the cupel of clay instead of sand would be more expensive and less convenient.

the earth repeatedly for several days, otherwise it would crack, and these cracks, which would become wider by the shrinking from heat, would afford a lodgment for some of the lead; an inconvenience which sand, even if rather loamy, does not present. It must also be remarked, that a cupel of clay would become too hard to admit of the excavation for carrying off the litharge; so that this part at least would require to be made of sand or ashes.

Two kinds of sand.

It will be advantageous to use two kinds of sand in forming a basin of the cupel, the one fine such as the founders' sand, and the other coarse. The latter may form the first stratum, which, after being well rammed with the implements used for this purpose, must be left about three inches in thickness. Upon this the fine and somewhat loamy sand is to be spread and rammed like the first. A slight degree of moisture must be used with both these, in order that they may more solidly adhere together. The lower stratum being more coarse, will facilitate the escape of the humidity,

Repair or renewal of the cupel.

It will not be necessary to disturb the lower stratum of sand when a new cupel is to be made; and even of this last that portion which has not imbibed any oxide may be used along with the new sand intended to be applied. The lower stratum must not be touched during this renewal, for fear of mixing coarse sand with the fine. This inconvenience may be guarded against by ramming upon the surface of the coarse sand a bed of a thin facing of ashes, at which the operator must stop when he takes away the upper stratum.

We have remarked that the founders' sand must be rather loamy, and that if it be not so, it will be necessary to add a small quantity of clay to render it adhesive; but as it is necessary that this clay should be equally diffused through the mass, it may be diffused in the water with which the sand is to be sprinkled, and the whole must be carefully mixed.

Absorption is of no advantage in the old process.

It might be objected, that since the cupels of sand do not absorb so much litharge as those of ashes, more time will be required to complete the refining, because the oxide instead of being in part absorbed, must by this new process be driven out of the furnace. This however is a subject which ought not to be considered as of any importance; for the blast of the bellows well directed will cause the oxide of litharge to flow out more abundantly through the gutter than if the absorption took place.

I have



I have seen operators in Germany, who, when they constructed their cupels, had the precaution to form a small circular cavity in the middle, the depth of which was proportioned to the quantity of silver, which from the assay they knew to be contained in the lead of one operation. By this contrivance there were no insulated grains of the metal left on the surface of their cupel, but the whole of the silver formed a perfectly round cake in the middle of the center excavation. I would advise the same ingenious expedient to be used in the cupels of sand.

Improvement. A  
Cavity to receive  
the silver.

I am well assured that the cupels I propose, if made with care and attention, will succeed perfectly, and that, independent of their convenience beyond the others, they will be found very economical. I am desirous that, for the advantage of metallurgy, this method should be generally used, and its benefits will prove that we ought not always to follow with servility the established usages nor the common working processes.

Conclusion.

## XI.

*Letter from Mr. JAMES STODART, explaining the Method of gilding upon Steel by Immersion in a Liquid, which has lately engaged the public Attention in various Articles of Manufacture.*

To Mr. NICHOLSON.

DEAR SIR,

A Considerable degree of public interest and curiosity has lately been excited by the exhibition of instruments of steel coated or gilt with gold. The discovery, although not altogether new, does not appear to be very generally known; and as its application to various manufactures promises to be both elegant and useful, I have, with a view of saving some time and expence to others who may be inclined to make further experiments, added a short account of a method which with me has succeeded perfectly well. I wish here in justice to observe, that this discovery of the method belongs more to my friend Mr. Hume, chemist, Long-Acre, than to myself. With that gentleman's kind assistance I had but few difficulties

Instruments of  
steel gilt with  
gold.

to

Account of the process. Three parts sulphuric ether are added to one of solution of gold. The gold is taken up by the ether.

The instrument to be dipped into the ethereal solution.

to overcome. The following is our method: To a saturated solution of gold in nitro-muriatic acid, add about three times the quantity of pure sulphuric ether: Agitate them together for a short time. The gold will soon be taken up by the ether in the form of a muriate, or nitro-muriate of gold, leaving the remaining acid colourless at the bottom of the vessel, which must now be drawn off by means of a stop-cock, or other similar contrivance. The acid being discharged, the instrument to be gilt having been previously well polished and wiped very clean, is to be dipped for an instant into the ethereal solution, and on withdrawing it, as instantly washed by agitation in clean water. This is essential to get clear of a small portion of acid necessarily taken up with the metal; and, if this be neatly done, the surface of the steel will be completely and very beautifully covered with gold. Some little degree of dexterity is required to perform the whole operation well.

Essential oils do not succeed well.

I have tried some of the essential oils, knowing that they will take the gold from nitro-muriatic acid; but as far as I went they did not apply for the purpose of gilding: and as I had found all I wanted in ether, I certainly did not prosecute the other experiments with much industry.

I remain, with much respect,

Dear Sir,

Your obliged servant,

J. STODART.

Strand, June 24, 1805.

## XII.

*On the peculiar Noise emitted by Water before it acquires the Temperature of boiling; which is commonly denoted by the Word Simmering. W. N.*

Introduction.

SOME time ago a philosophical friend who favoured me with a visit, mentioned in conversation that the simmering of water before it boils had formed the subject of enquiry between himself and other curious examiners of natural appearances, as being a fact not yet well explained. It seemed to me very remarkable, as I dare say it will to my readers, that any  
of

of the facts relating to the boiling of water, concerning which so much has been said and written, should still remain in obscurity. Having, myself, been in the habit of considering it as the consequence of a rapid escape of interspersed air from the heated water, I requested he would say why he thought the phenomenon repugnant to that supposition. My friend proceeded in reply to state, that when water is first put on the fire, in a metallic vessel, the vessel itself gradually becomes lined with bubbles; that these bubbles become detached and rise, so that the whole surface, or at least the bottom, becomes clear again; that, soon afterwards a rattling noise is heard, sharp and metallic, which encreases in loudness until it almost perfectly resembles the pouring of small shot into the vessel; that the fluid continues in a state of tranquillity and transparency during this state; and lastly, that when the noise is loudest the state of ebullition suddenly comes on, the peculiar noise of simmering ceases all at once, and nothing is heard but the soft and moderate noise of aquatic agitation as long as the boiling lasts.

Particular description of the act of simmering.

This account, so remarkable for its precision and accuracy, shewed clearly that my notion, which I believe is the common opinion, was ill founded. After a little meditation, it appeared evident to me that the noise of simmering must arise from the collapshon of steam bubbles, formed at the bottom of the vessel, and condensed almost instantly upon their ascent in the fluid not yet heated to the boiling point. In support of this opinion I shewed him a common experiment with the water-hammer. This instrument, which is made and sold by the glass-blowers and barometer-makers, consists of a tube, nearly a foot in length and about three quarters of an inch in diameter, terminating in a globe of about two inches in diameter; the other end of the tube being closed. The outer extremity of the globe ends in a capillary tube, through which as much water has been introduced as rather more than fills the globe itself. This water has been boiled in the vessel or instrument, and at the time of boiling, when all the internal cavity not containing water was filled with steam to the almost total exclusion of air, the capillary aperture was hermetically closed. The instrument thus completed is found when cold to contain water and a space nearly vacuous, and the experiment from which it derives its name

It seems to arise from the sudden production and condensation of steam.

Description of the water-hammer.

of



Singular noise of of the water-hammer, is that of agitating its contents. The water agitated in remarkable effect it exhibits is, that the parts of the water vacuo. strike against the glass and against each other, with the sharp

Another experi- noise usually produced by the collision of hard bodies. Another ment of steam experiment, which I alluded to, is that, if the ball be held produced and condensed downwards so as to become filled with the water, and it be twelve times in a then slowly raised up, so as to bring the tube nearer to the second, horizontal position, the heat of the hand which holds the

applied to explain collapfions occur in every second of time. The fact, and the the effect of obvious remarks I made upon it, convinced my friend that I simmering. had suggested the proper explanation.

#### Objections.

It was my intention however to have heated some water in a vessel in order to observe and ascertain the progressive appearances, but I had not done it when I again had the pleasure to meet this intelligent observer. He complimented me upon the ingenuity of my solution, but having himself since repeated the experiment of boiling water, it seemed from his report that ingenuity was all the value it could claim. "Take a bright tin vessel" said he "and heat water in it; you will hear the noise, but no bubbles are to be seen."

I took the earliest opportunity of making some experiments, the particulars and results of which are as follow :

Exp. 1. Water was heated in a glass retort. The simmering was attended with steam bubbles.

Exp. 1. A small glass retort, the body of which is about two inches in its shortest or horizontal diameter, was suspended so that its neck was elevated about twenty degrees above the level. Water was then poured in to fill the body and the greatest part of the neck. My intention in filling the neck was, that I might be able to observe whether any greater or more sudden rise took place before the period of boiling than the well known expansion of the fluid. A small spirit-lamp was placed beneath the bulb. The coldness of the water in the vessel immediately condensed a portion of the water which issued

issued from the flame itself, and formed drops on the outside. As the included water became heated this condensed water evaporated, and left the surface again clear; and at this period the disseminated air began to separate and gave a dusty appearance to the inner surface, which lasted about three minutes. At the end of this time the inner surface began to clear; the peculiar noise of simmering was heard; and bubbles were seen suddenly appearing and collapsing; the retort itself being agitated and the surface of the water rising and falling by starts. The bubbles were pointed at top, somewhat resembling small flames suddenly appearing and vanishing at different parts of the surface. In the course of one minute they grew larger and larger and collapsed at greater heights, until at length they escaped through the fluid without being condensed. This was the instant of ebullition or boiling and at this period the noise of simmering ceased and that of boiling was heard.

*Exp. 2.* As this effect appears to arise decidedly from the upper water being colder than that near the bottom of the vessel, it was natural to infer that the appearances would be different according to its figure and magnitude. I therefore took a bolt-head, or spherical glass body, with a straight neck: Its diameter was four inches; and when it was filled, a column of water eight inches long stood in its upright neck. The thickness of the glass at its bottom was considerable. At 35 minutes after three the lamp was lighted. At 40 minutes bubbles of gas rose singly, and very little of the dusty appearance was seen. At 58 minutes the noise of simmering began, and the collapsing bubbles were plentiful and distinct. Little streams or fountains of steam rose from particular points and were condensed; and some globes of half an inch diameter ascended clear of the bottom and collapsed in the fluid above. At one minute after four the bubbles reached the top of the fluid without collapsing, and at this time the noise of simmering ceased. The lamp was then blown out.

*Exp. 2.* Water was heated in a larger glass vessel. The effects were rather more evident.

*Exp. 4.* A bright copper hemisphere, four inches in diameter, was filled with water at the temperature of 60 degrees. At four hours three minutes the lamp was lighted and water became condensed on the outside. At five minutes the inside surface had a dusty appearance, from bubbles of air immediately over the flame. The temperature was then 110°, and the

*Exp. 4.* The experiment was repeated in a copper vessel.

the outside dry. At six minutes bubbles of gas or air were detached and rose, the temperature being  $126^{\circ}$ , and the vapour of steam being visible from the surface of the water. At eight minutes the inside surface was coated with large bubbles or beads of air; temperature  $150^{\circ}$ . At nine minutes, temp.  $165^{\circ}$ , much vapour. At  $9\frac{1}{2}$  min. temp.  $175^{\circ}$ , the bottom was clear of bubbles, and the noise of simmering began. At 10 min. temp.  $184^{\circ}$ , the steam collapsing bubbles were very evident, though, from their pointed shape, they were not immediately obvious to an observer looking straight down into the shining vessel. At 11 min. temp.  $180^{\circ}$ , noise very loud and bubbles more and larger. At 12 min. temp.  $185^{\circ}$ , some of the bubbles broke at the surface, and the noise was less. At  $12\frac{1}{4}$  min. temp.  $204^{\circ}$ , the water boiled, and the simmering noise was succeeded by that of boiling. At 14 min. the lamp was extinguished.

Exp. 5. Water already heated does not simmer so much or so loudly as water quickly raised from a low temperature.

Exp. 5. The water was suffered to cool to  $170^{\circ}$ , and the lamp was then lighted again, namely, at four hours 18 minutes. At 20 min. temperature  $180^{\circ}$ , the simmering began; but not till after the steam bubbles were seen very large: and soon afterwards, at  $204^{\circ}$ , they rose through the fluid, and the boiling took place by fountains or streams of bubbles rising from particular points. The simmering noise in this experiment was much less than before.

The thermometer was placed horizontally, with the greatest part of its tube, and part of its bulb, out of the water. When the bulb was plunged in the water, it shewed  $208^{\circ}$ .

Conclusion.

From these facts it appears to be clearly established, that the cause first mentioned, namely, the condensation of steam bubbles in their ascent through the cold fluid above, is the occasion of the noise of simmering. In the fifth experiment the superincumbent water was hotter than that beneath, and consequently the simmering could not be produced but by steam at a higher temperature, and even then the collapse of the water was less sudden and the noise less loud.



## XIII.

*Experiments on Wootz. By Mr. DAVID MUSHET. From the Philosophical Transactions, 1805.*

THE following experiments were made at the request of Sir Joseph Banks, on five cakes of wootz, with which he supplied me for that purpose. As the cakes, which were numbered 1, 2, 3, 4, 5, were not all of the same quality, it will be proper first to describe the differences observable in their external form and appearance.

Experiments on  
five cakes of  
wootz.

No. 1 was a dense solid cake, without any flaw or fungous appearance upon the flat, or, what I suppose to be, the upper side. The round or under surface was covered with small pits or hollows, two of which were of considerable depth; one through which the slit or cut had run, and another nearly as large towards the edge of the cake. These depressions, the effects, as I suppose, of a species of crystallization in cooling, were continued round the edges, and even approached a little way upon the upper surface of the wootz.

Description of  
the cake, No. 1.

The cake was a quarter of an inch thicker at one extremity of the diameter than at the other, from which I infer, that the pot or crucible, in which this cake had been made, had not occupied the furnace in a vertical position. Its convexity, compared to that of the other five, was second. Upon breaking the thin fin of steel, which connects the half cakes together, I found it to possess a very small dense white grain. This appearance never takes place but with steel of the best quality, and is less frequent in very high steel, though the quality be otherwise good.

Fractured very  
steely.

Upon examining the break with attention, I perceived several laminæ and minute cells filled with rust, which in working are never expected to unite or shut together. The grain otherwise was uniformly regular in point of colour and size, and possessed a favourable appearance of steel.

No. 2. This cake had two very different aspects; one side was dense and regular, the other hollow, spongy, and protuberant. The under surface was more uniformly honey-combed than No. 1; the convexity in the middle was greater, but towards the edges, particularly on one side, it became flatter. The grain exposed by breaking was larger, bluer in colour, and

Description of  
the cake of  
wootz, No. 2.

and more sparkling than No. 1. In breaking, the fracture tore but slightly out, and displayed the same unconnected laminæ with rusty surfaces, as was observed in No. 1. Beside these, two thin fins of malleable iron projected from the unsound side, and seemed incorporated with the mass of steel throughout. Towards the centre of the break, and near to the excrescence common to all the cakes, groups of malleable grains were distinctly visible. The same appearance, though in a slighter degree, manifested itself in various places throughout the break.

Cake of wootz,  
No. 3.

No. 3. The upper surface of this cake contained several deep pits, which seemed to result from the want of proper fluidity in fusion. They differed materially from those described upon the convex sides of No. 1 and 2, and were of that kind that would materially effect the steel in forging.

The under or convex side of this cake presented a few crystalline depressions, and those very small; the convexity was greater than that of No. 1 and 2, the fracture of the fin almost smooth, and only in one place exhibited a small degree of tenacity in the act of parting. In the middle of the break, about half an inch of soft steel was evident; and in different spots throughout numerous groups of malleable grains, and thin laminæ of soft blue tough iron made their appearance.

Cake of wootz,  
No. 4.

No. 4. Was a thick dense cake possessed of the greatest convexity; the depressions upon the under side were neither so large, nor so numerous as those in No. 1 and 2, nor did they approach the upper surface of the cake further than the acute edge. This surface had the most evident marks of hammering to depress the feeder, or fungous part of the metal, which in the manufacturing seems the gate or orifice by which the metal descends in the act of gravitation.

The break of this cake, however favourable as to external appearance, was far from being solid. Towards the feeder it seemed loose and crumbly, and much oxidated. The grain divided itself into two distinct strata, one of a dense whitish colour, the other large and bluish, containing a number of small specks of great brilliancy. Several irregular lines of malleable iron pervaded the mass in various places, which indicated a compound too heterogeneous for good steel.

Cake of wootz,  
No. 5.

5th cake. This was materially different in appearance from any of the former. It had received but little hammering, yet

was smooth and free from depressions, or honey-comb on both surfaces. The feeder, instead of being an excrescence, presented a deep concave beautifully crystallized.

In breaking, the fracture tore out considerably, but presented a very irregular quality of grain. That towards the under surface was small and uniform, but towards the flat or upper surface it increased in size, and in the blueness of its colour, till it passed into the state of malleable iron.

The break of this steel, though apparently soft, was the least homogeneous of the whole, and throughout it presented a very brilliant arrangement of crystal, which in other steel is always viewed with suspicion.

#### *General Remark.*

Uniformly the grain and density of the wootz are homogeneous, and free from malleable iron towards the under or round surface; but always the reverse towards the feeder or upper side.

#### *Remarks in Forging.*

No. 1. One-half of the cake was heated slowly by an annealing heat to a deep red, and put under a sharp broad-mouthed chissel with a small degree of taper. It cut with difficulty, was reheated, and cracked a little towards one end of the slit or cut originally in the cake.

The appearances on forging the cake of wootz, No. 1.

The heat in this trial was so moderate, that I was afraid that the crack had arisen from a want of tenacity, occasioned by the heat being too low.

The other half was heated a few shades higher, and subjected to the same mode of cutting; before the chissel had half way reached the bottom, the piece parted in two in the direction of the depression made by the cutting instrument. The additional heat in this instance proved an injury, while the cracking of the steel in both cases, particularly the former, was a certain proof of the abundance, or rather of the excess of the steely principle.

The fractures of both half cakes, now obtained for a second time, were materially different from that obtained by the simple division of the cake. The grain was nearly uniform, distinctly marked, but of too gray a colour for serviceable steel. Two of the quarters being drawn into neat bars under hand hammers



The appearances on forging the cake of wootz, No. 1.

mers at a low heat, one of them contained a number of cracks and fissures. The fracture was gray, tore out a little in breaking, but was otherwise yolkly and excessively dense. A small bar of penknife size was improved greatly in drawing down, and had only one crack in thirteen inches of length. The grain and fracture were both highly improved by this additional labour; the tenacity of the steel was greater, and it stood firmly under the hammer at a bright red heat.

The other two quarters of this cake were squared a little, and successively put under a tilt hammer, of two hundred weight, going at the rate of three hundred blows per minute, and drawn into small penknife size. One of the bars from an outside piece, always the most solid, was entirely free from cracks, and had only one small scale running upon one side.

These bars exhibited a tougher break, than those drawn by hand; the colour was whiter, and the grain possessed a more regular and silky appearance.

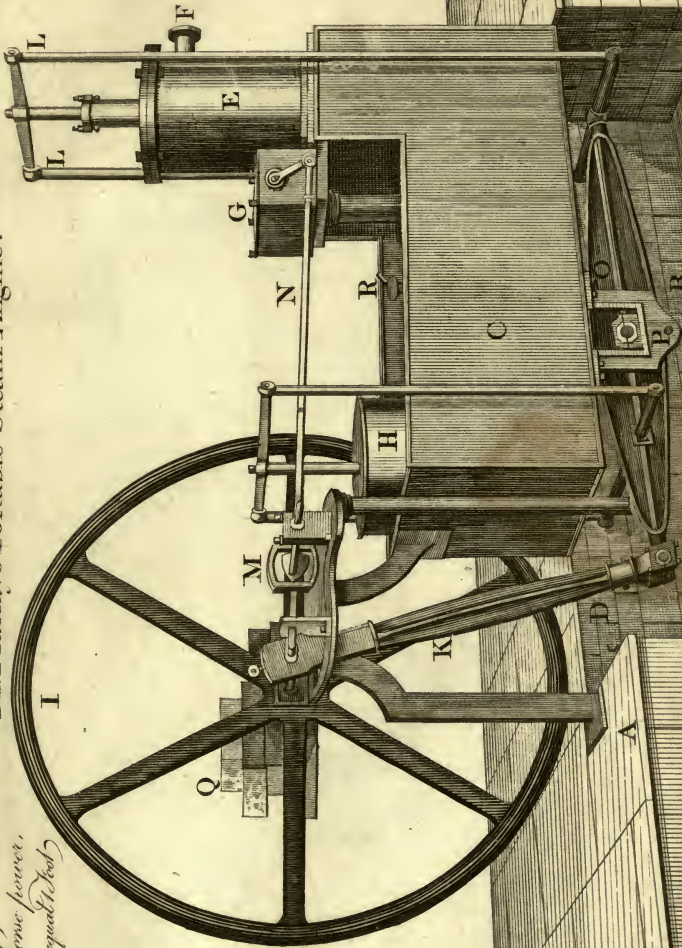
(To be continued.)

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\* \* I have received a letter from Mr. BOSWELL, in which he expresses an opinion, that it is unfair in the Old Correspondent whose letter appeared in our last Number to have applied Mr. B's apology to one particular part of his paper; as he conceives, that it ought to have been considered as indicating the spirit in which his whole communication was written. I have inserted this short notice out of respect to the writer; but have declined inserting the letter itself, because the controversy can have no farther importance to the readers of this Journal, after the subject itself has been exhausted.

M. Murray's Portable Steam Engine.

*Four Horse power.  
Stroke  $\frac{1}{2}$  Inch equal Wheel*

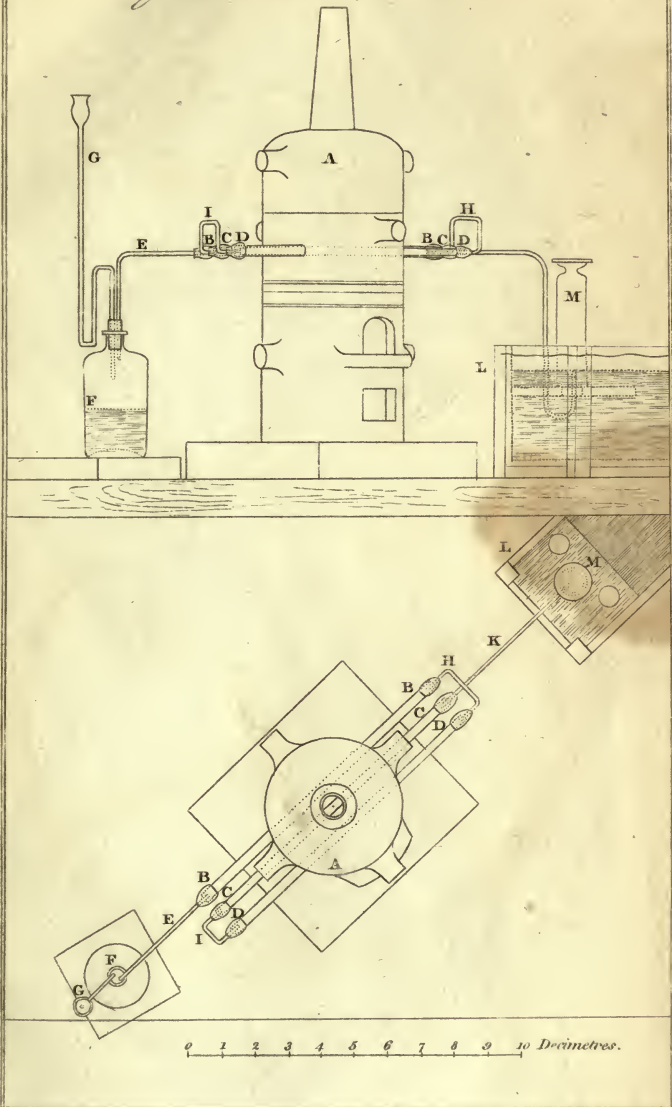


Original owned by Albert A. 10/17/94





*Apparatus of Baruel for making the  
Gaseous Oxide of Carbon.*



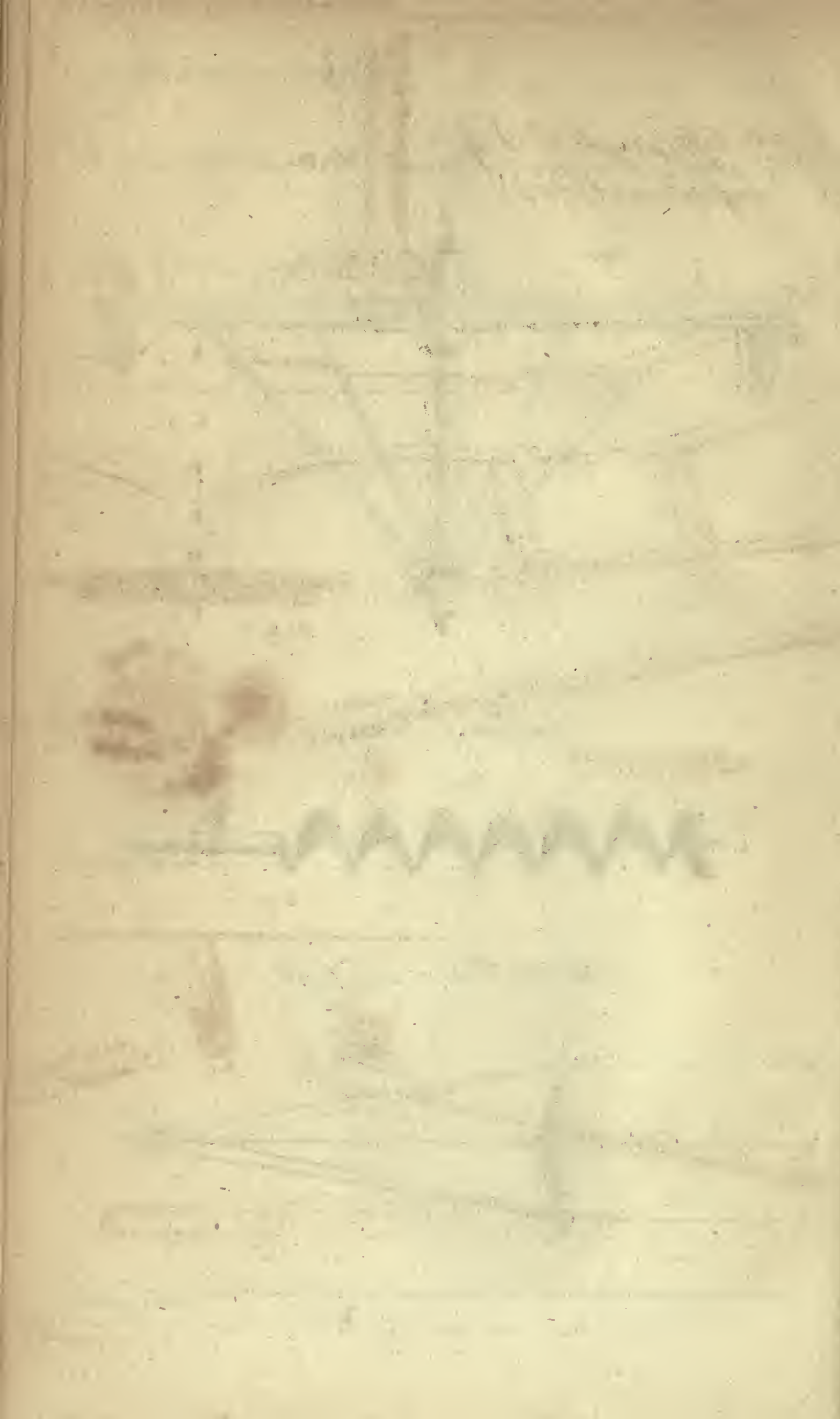






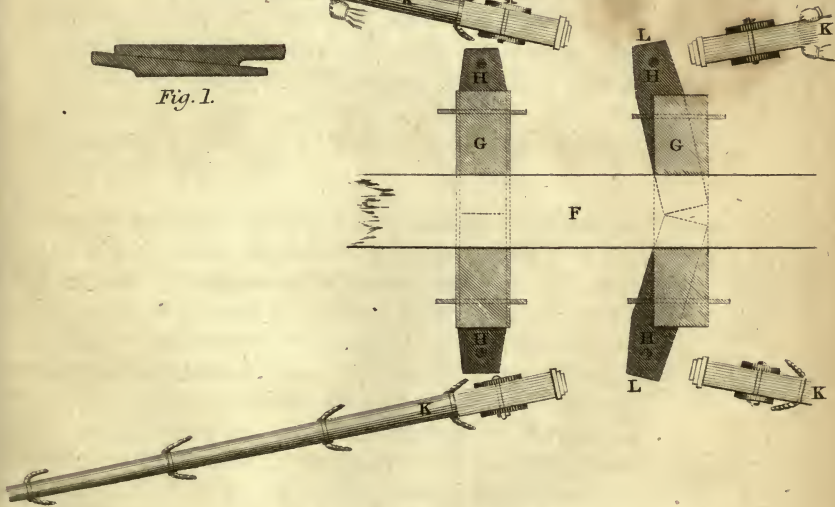
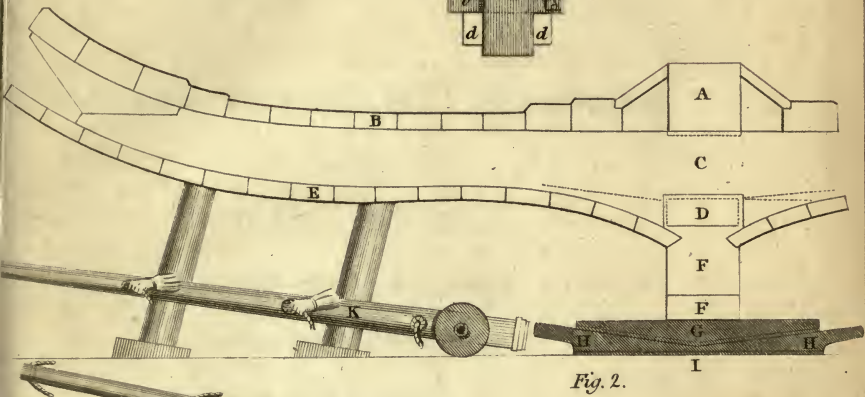
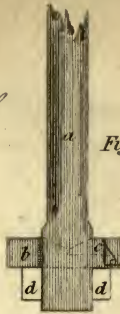




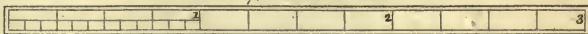




*Mr. Leppings Method of  
suspending Ships.*



Scale  $\frac{3}{4}$  of an Inch to a Foot.





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A  
JOURNAL  
OF  
NATURAL PHILOSOPHY, CHEMISTRY,  
AND  
THE ARTS.

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AUGUST, 1805.

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ARTICLE I.

*An Account of some new Experiments which prove that the Temperature at which the Density of Water is a Maximum, is several Degrees of the Thermometer above the freezing Point. By BENJAMIN COUNT OF RUMFORD, V. P. R. S. Foreign Associate of the National Institute of France, &c. &c. Received July 16, 1805, from the Author; with a Letter dated Munich, 25th June, 1805.*

IN my seventh essay, in which I have treated of the propagation of heat in fluids, and also in a paper published in the Philosophical Transactions for the Year 1804, Part I.; in which I have given an account of a curious phenomenon frequently observed on the Glaciers of Chamouny, I have ascribed the melting of ice which is placed (by design, or by accident) below the surface of ice-cold water, to currents of warmer water, which, in certain cases, are supposed to descend in that ice-cold liquid: but as this supposed fact has lately been called in question by several persons, and as the explanations I have founded on it must fall to the ground, unless it can be supported, I have been induced to re-consider the matter, and to give it a careful and thorough investigation.

Phenomena which have been explained from the maximum density of water being at higher than freezing temperature:

the fact questioned.



It was first announced by De Luc.

The fundamental fact on which the supposition is grounded, which was announced many years ago by Mr. De Luc; namely, that the temperature at which the density of water is a maximum, is considerably higher than that at which that fluid freezes, is indeed so very extraordinary, and appears to be the cause of so many interesting phenomena, that too much pains cannot be taken to put it beyond doubt.

New method of proving it.

As the methods hitherto used for determining that important point have, by some at least, been considered as insufficient, I shall take the liberty to propose another, by which the fact in question may, I think, be demonstrated directly; and without any nice calculations, or any very difficult or delicate experiments.

Let the following experiments, (which it will be easy to repeat) speak for themselves.

Apparatus. In the middle of a thin cylindrical brass vessel, a thin brass cup is supported.

Having provided a cylindrical vessel, (A Fig. 1. Plate XII.) open above, made of thin sheet brass, which is  $5\frac{1}{2}$  inches in diameter, and four inches deep, supported on three strong legs,  $1\frac{1}{4}$  inches high; I placed in it a thin brass cup, (B) two inches in diameter at its bottom, (which is a little convex downwards)  $2\frac{8}{10}$  inches wide at its brim; and  $1\frac{3}{10}$  inches deep; which cup stands on three spreading legs made of strong brass wire, and of such form and length, that when the cup is introduced into the cylindrical vessel, it remains firmly fixed in the axis of it, and in such a situation, that the bottom of the cup is elevated just  $1\frac{1}{4}$  inches above the bottom of the cylindrical vessel.

In the middle of the brass cup is supported

In the middle of this cup there stands a vertical tube of thin sheet brass,  $\frac{1}{2}$  an inch in diameter, and  $\frac{6}{10}$  of an inch in length, open above, which serves as a support for another smaller cup (C) which is made of cork; the brim of which is on the same horizontal level with the brim of the larger brass cup, in which it is placed.

an hemispherical cup of cork,

This cork cup, which is spherical, (being something less than half of an hollow sphere) is one inch in diameter at its brim, measured within  $\frac{4}{10}$  of an inch deep, and  $\frac{1}{4}$  of an inch in thickness. It is firmly attached to the vertical tube on which it stands, by means of a cylindrical foot,  $\frac{1}{2}$  an inch in diameter, and  $\frac{1}{4}$  of an inch high; which, when some force is employed, enters into the opening of the vertical tube.

On

On one side of this cork cup there is a small opening, which receives, and in which is confined the lower extremity of the tube of a small mercurial thermometer, (D).. The bulb of this thermometer, which is spherical, is just  $\frac{3}{10}$  of an inch in diameter, and it is so fixed in the middle of the cup, that its centre is just  $\frac{1}{4}$  of an inch above the bottom of the cup; consequently it does not touch the cup any where, nor does any part of it project above the level of its brim.

The tube of this thermometer, which is six inches in length, has an elbow near its lower end, at the distance of one inch from its bulb, which elbow forms an angle of about 110 degrees, and the thermometer is so fixed in the cup, that the short branch of its tube, namely, that to the end of which the bulb is attached, lies in an horizontal position, while the longer branch (to which a scale, made of ivory, and graduated according to Fahrenheit is affixed,) projects obliquely upwards and outwards, in such a manner that the freezing point of the scale lies just above the level of the top of the cylindrical vessel in which the cups are placed.

The cork cup, which was turned in the lathe, is neatly formed, and in order to close the pores of the cork, it was covered, within and without, with a thin coating of melted wax; which was polished after the wax was cold.

The thermometer was fixed to the cork cup by means of wax, and in doing this, care was taken to preserve the regular form of the cup, both within and without.

The vertical brass tube which supports this cup in the axis of the brass cup, is pierced with many holes, in order to allow a free passage into it, and through its sides, to the water employed in the experiments.

Having attached about six ounces of lead to each of the legs of the brass cup, in order to render it the more steady in its place, it was now introduced with its contents, into the cylindrical vessel, and the vessel was placed in an earthen basin (E) seven inches in diameter below, 11 inches in diameter at its brim, and five inches deep, and was surrounded on all sides with pounded ice.

Several flat cakes of solid ice were now put into the cylindrical vessel, and fastened down upon its bottom, and under the bottom of the brass cup, and a circular row of other long pieces of ice were placed, in a vertical position, round the

having in its center the bulb of

Other particulars.

This apparatus was placed in a pan, and surrounded with ice.

Ice was placed and secured in the cylindrical vessel; but not in the cups, and ice-cold water was poured

into all the remaining space.

outside of the brim of the brass cup, between it and the vertical side of the cylindrical vessel, which vertical pieces of ice reached upwards to within about  $\frac{1}{10}$  of an inch of the top or brim of the cylindrical vessel; and when this was done, ice-cold water was poured into this vessel till the surface of that cold fluid stood just one inch above the level of the brim of the cork cup.—Both cups were of course submerged and filled with ice-cold water, and surrounded on all sides by solid cakes of ice.

The cups remained thus submerged for one hour.

After things had remained in this situation more than an hour, during which time the cold-water in the cylindrical vessel, and that in the cups, was frequently agitated with the soft end of a strong feather, and the cups and the water in every part of the vessel appearing to be exactly at the temperature of freezing: I proceeded to make the following decisive experiment.

#### Experiment. No. 1.

Experiment I.  
A cone of metal at  $42^{\circ}$  was plunged in the ice-cold water, just above the cork cup:

A solid ball (F) of tin having been provided, two inches in diameter, with a cylindrical projection on the lower side of it, one inch in diameter, and half an inch long, ending in a conical point, which projected (downwards) half an inch farther; this ball (to which was fixed a strong iron wire six inches in length, which served as a handle to it,) having been made to acquire the temperature of  $42^{\circ}$  F. by keeping it immersed near half an hour in a large quantity of water at that temperature, was placed as expeditiously as possible over the middle of the cork cup, and in such a situation that the whole of the descending conical point (half an inch in length) was immersed in the ice-cold water in the cylindrical vessel, the extremity of that point being just half an inch perpendicular above the upper side of the bulb of the thermometer which lay in the cork cup.

It was foreseen, that if the water in contact with the cone became denser by the heat, it would descend and raise the thermometer.

I knew that the particles of ice-cold water which were thus brought into contact with the conical point, could not fail to acquire some small degree of heat from that relatively warm metal; and I concluded, that if the particles of water so warmed should in fact become *heavier* than they were before, in consequence of this small increase of temperature, they must necessarily *descend* in the surrounding lighter ice-cold liquid, and as the heated metallic point was placed directly



directly over the cork cup, and fixed immoveably in that situation, I foresaw that the descending current of warm water must necessarily fall into that cup, and at length fill it, and that the presence of this warm water in the cup would be announced by the rising of the thermometer.

The result of this very interesting experiment was just what I expected: the conical metallic point had not been in contact with the ice-cold water more than 20 seconds, when the mercury in the thermometer began to rise, and in three minutes it had risen three degrees and a half, namely, from  $32^{\circ}$  to  $35\frac{1}{2}^{\circ}$ , when five minutes had elapsed, it had risen to  $36^{\circ}$ , when an end was put to the experiment.

Another small thermometer, placed just below the surface of the ice-cold water, and only  $\frac{2}{15}$  of an inch from the upper part of the conical point, on one side of it, did not appear to be sensibly affected by the vicinity of that warm body.

A third thermometer, the bulb of which was placed in the brass cup, just on the outside of the cork cup, and on a level with its brim, shewed that the water which immediately surrounded the cork cup, remained constantly at the temperature of freezing, during the whole time that the experiment lasted.

As I well knew, from the results of the experiments on the propagation of heat in a solid bar of metal, of which an account has been given in a memoir presented to the first class of the National Institute of France, on the 7th of May 1804, that the ice-cold water in this experiment could not possibly acquire from a contact with the conical metallic point, a temperature so high as that of  $42^{\circ}$ , I was by no means surprised to find that the thermometer belonging to the cork cup rose no higher.

In order to see if it could not be made to rise not only higher, but also more rapidly, by employing the metallic ball heated to such a temperature as it might by supposed would be sufficient to heat those particles of ice-cold water which should come into contact with its conical point, to the temperature at which the density of water is supposed to be a maximum, I made the following experiments.

#### Experiment. No. 2.

Having removed the ball, I gently brushed away the warm water, which, in the last experiment had been lodged in the cavity

It did, in fact, raise the thermometer in the cup;

but it did not affect a thermometer near the surface of the water;

nor another near the outside of the cup.

The metal at  $42^{\circ}$  could not raise the water to the same temp.

A somewhat greater heat proposed to be applied.

Experiment 2. The former experiment was

repeated; the cone being heated to 60°.

cavity of the cork cup, (and which still remained there, as was evident from the indication of the thermometer belonging to the cup,) I placed several small cakes of ice in the cylindrical vessel, which ice floating on the surface of the water in the vessel, prevented that water from receiving heat from the air of the atmosphere, which at that time was at the temperature of 76° F. And as the cork cup had been a little heated by the warm water in the foregoing experiment, time was now given it to cool.

As soon as the cup, and the whole mass of the water in the cylindrical vessel appeared to have acquired the temperature of freezing, I carefully removed the cakes of ice which floated on the surface of the water, and introduced once more the projecting conical point belonging to the metallic ball into the ice-cold water in the vessel, placing it exactly in the same place which it had occupied in the foregoing experiment; but this ball, instead of being at the temperature of 42° F. as before, was now at the temperature of 60° F.

The effect was more rapid and more considerable.

The result of this experiment was very striking, and if I am not much mistaken, affords a direct, unexceptionable, and demonstrative proof, not only that the maximum of the density of water is in fact at a temperature which is several degrees above the point of freezing; but also, that warm currents do actually set downwards in ice-cold water, whenever a certain small degree of heat is applied to the particles of that fluid which are at its surface.

Particulars,

The conical metallic point had been in its place no more than ten seconds when I distinctly saw that the mercury in the thermometer belonging to the cork cup was in motion; and, when 50 seconds had elapsed, it has risen four degrees, viz. from 32° to 36°.

When two minutes and a half had elapsed, (reckoning from the moment when the metallic point was introduced into the cold water,) the thermometer had risen to 39°, and at the end of six minutes to 39 $\frac{7}{8}$ °, when it began to fall; but very slowly however, for at the end of eight minutes and a half it was at 39 $\frac{3}{4}$ °.

A thermometer near the outside of the cup was not affected.

A small mercurial thermometer, the bulb of which was placed on one side of the cork cup, at the distance of about  $\frac{2}{10}$  of an inch from it, shewed no signs of being in the least affected by the heat communicated to the ice-cold water by the metallic ball.

This

This experiment was repeated three times the same day, (the 13th of June 1805,) and always with nearly the same results.

The mean results of these four experiments were as follows: Tabulated results of the experiments repeated, with the cone at a low heat, as shewn by the thermometer.

Time elapsed reckoned from the beginning of the experiment.		Temperature of the water in the cork cup, as shewn by the thermometer.						
Min.	Sec.							
0	0	-	-	-	-	-	-	32°
At 0	10	began to rise	-	-	-	-	-	32+
At 0	23	had risen to	-	-	-	-	-	33
0	28	-	-	-	-	-	-	34
0	35	-	-	-	-	-	-	35
0	48	-	-	-	-	-	-	36
1	3	-	-	-	-	-	-	37
1	35	-	-	-	-	-	-	38
2	32	-	-	-	-	-	-	39
3	41	-	-	-	-	-	-	39½
4	48	-	-	-	-	-	-	39¾
6	5	-	-	-	-	-	-	39¾

As I had found by some of my experiments made in the year 1797, of which an account is given in my seventh essay, part I. that water at the temperature of about 42° F. and consequently what we should call very cold, melted considerably more ice, when standing on it, than an equal quantity of boiling hot water, in the same situation, I was very curious to see whether the thermometer, the bulb of which lay in the cork cup, would not also be less heated by the ball when it should be applied *very hot*, to the surface of the water, than when its temperature was much lower.

To determine that point I made the following experiment.

### Experiment. No. 3.

The cylindrical vessel with its contents having been once more reduced to the uniform temperature of freezing water, the metallic ball was heated in boiling water, and being as expeditiously as possible taken out of that hot liquid, its projecting conical point was suddenly submerged in the ice-cold water, as in the former experiments.

The



The result of this experiment was very interesting, and it seems to me to throw much light on the subject of these investigations.

Effect on the thermometer in the cup much els. It was not till 50 seconds had elapsed that the thermometer began to shew any signs of rising, and at the end of one minute and seven seconds it had only risen two degrees.

In the foregoing experiments, when the metallic ball was so much colder, the thermometer began to rise in ten seconds, and at the end of one minute and three seconds it had risen five degrees.

This difference is very remarkable ; and if it does not prove the existence, and great efficacy of currents, in conveying heat in fluids, I must confess that I do not see how the existence of any invisible mechanical operation, the progress of which does not immediately fall under the cognizance of our senses, can ever be demonstrated

As the experiment made with the ball heated in boiling water appeared to me to be very interesting, I repeated it twice, and its results were always nearly the same.

Tabulated results of experiments repeated ; with the cone at a considerable heat. The mean results of these three experiments were as follows :

Time elapsed, reckoned from the beginning of the experiment.		Temperature of the water in the cork cup, as shewn by the thermometer.						
Min.	Sec.							
0	0	-	-	-	-	-	-	32°
At 0	50	the thermometer begun to rise.						
At 1	2	had risen to						
1	7	-	-	-	-	-	-	33
1	18	-	-	-	-	-	-	34
2	2	-	-	-	-	-	-	35
3	2	-	-	-	-	-	-	36
4	17	-	-	-	-	-	-	36½
6	12	-	-	-	-	-	-	37
7	17	-	-	-	-	-	-	38
9	0	-	-	-	-	-	-	38⅛
12	0	-	-	-	-	-	-	38¼
14	0	-	-	-	-	-	-	38½

Comparative view of the experiments. By comparing the mean results of these experiments with the mean results of those in which the ball was at the temperature

rature of  $60^{\circ}$ , we may see how much more rapidly the thermometer in the cork cup acquired heat when the metallic ball was *relatively cold* than when it was at the temperature of boiling water; and it is more than probable that it was not till after the conical metallic point had been considerably cooled, by a contact with the ice-cold water, that those streams of moderately warmed water began to descend from it, by which the thermometer was at length heated.

In the experiments made with the ball heated in boiling water, a small thermometer, placed just under the surface of the ice-cold water, on one side of the metallic point, began to rise very rapidly as soon as the hot ball was fixed in its place; but another thermometer placed about half an inch lower, on one side of the cork cup, remained to all appearance at perfect rest, from the beginning of the experiment to the end of it.

When the very hot cone was used, a thermometer at the surface was heated; —but a lower one was not.

The explanation of all these appearances is so easy that it would be a waste of time to say much on that subject. It may however be useful to recapitulate the principal phenomena, and shew in what manner they tend to establish the facts which they are brought to prove.

Every body must see, at the first glance, that in all these experiments the heat which caused the thermometer to rise was *carried down* into the cork cup by descending currents of warm water; and it is evident that water which descends must of necessity be specifically heavier than that in which it descends.

That the slightly heated water descends;

From the results of these experiments we may conclude that the density of water is a maximum when that fluid is at a temperature somewhat lower than that of the *fortieth degree* of Fahrenheit's thermometer.

—and the greatest density is rather below  $40^{\circ}$ .

In all the foregoing experiments, more or less warm water descended through the ice-cold water into the cork cup; but the results of the experiments which were made with the metallic ball heated in boiling water, shew evidently that when the particles of water at or near the surface of a quiescent mass of ice-cold water, are by any means heated to a temperature several degrees above that of  $40^{\circ}$  of the scale of Fahrenheit, such particles, so heated, become specifically lighter than ice-cold water, and consequently cannot descend in that cold and denser liquid.

Hotter water is lighter and ascends.

With the hot cone some of the fluid ascended and some descended.

In the experiments in question, (with the hot ball) some of the particles of the water, namely, those which came first into contact with the conical metallic point, were heated to a higher temperature than that at which they were disposed to sink in ice-cold water; and these rose and spread themselves over the surface of the cold liquid; but others, which happened to acquire less heat, descended in it, and after filling the cork cup, overflowed, no doubt its brim, and then descending to the bottom of the brass cup, and coming into contact with that ice-cold metal, were there cooled, and there remained at rest.

Cork a good non-conductor.

As cork is an excellent non-conductor of heat, the warm water accumulated in the cork cup during an experiment retained its heat a long time after the heated ball was removed, notwithstanding its being surrounded on all sides, and even covered immediately by ice-cold water; (which by the by is a pretty strong proof that water is by no means a good conductor of heat) care however was taken, not only to remove this warm water after each experiment, by brushing it away with the soft end of a strong feather or quill, but also to cool the cup, and reduce it to the temperature of freezing water; which last operation was found to be much accelerated by brushing it out frequently with the feather. In order that this feather might itself be ice-cold, it was suffered to remain constantly in the ice-cold water, in the cylindrical vessel.

Apparatus by which the cone was fixed.

I must not forget to give an account of the means used for fixing the metallic ball in its place. This was done in a very simple and effectual manner. A slip of strong tin (G H) six inches long, and  $2\frac{1}{4}$  inches wide, with a circular hole in the middle of it, one inch in diameter, being laid horizontally on the top or brim of the cylindrical vessel, in such a manner that the center of that circular hole coincided with the axis of the cylindrical vessel, the short cylindrical projection belonging to the ball being introduced into that hole, the ball was firmly supported in its proper place.

The quantity of ice-cold water in the cylindrical vessel was so regulated that the whole of the conical point being submerged, the surface of the water was on a level with the lower end of the cylinder, or, which is the same thing, with the base of the inverted cone.



When not only the whole of the conical point, but a part also of the short cylinder were immersed in the ice-cold water, the warmed water appeared to be thrown into eddies in its descent, which dispersing about prevented its falling regularly in one continued stream into the cork cup.

The conical figure preterable for the immersed metal.

To conclude, I would just observe, that although the foregoing experiments appear to me to be perfectly unexceptionable, and that their results afford demonstrative proof of the facts which they were contrived to establish; yet, when attempts are made by experiments similar to these, to determine whether heat can be made to pass *downwards* in water which is at a higher temperature than that at which its density is a maximum, difficulties occur which appear to me to be quite insurmountable.

It is not easy to make conclusive experiments at elevated temperatures :

The fluidity of water is so perfect, or the mobility of its particles so great, that the liquid at the surface, which is first heated and rarefied, immediately spreads far and wide, and meeting with the sides of the containing vessel, heats them, and this heat, so acquired, making its way downwards (as well as upwards) in the solid substance of which the vessel is constructed, raises the temperature of the lower strata of the fluid in contact with it, which moving towards the axis of the vessel, communicates heat to a thermometer placed there, below the surface of the water.

For the sides of the vessel greatly modify the effects by conducting the heat, &c.

That these various operations do in fact take place, nobody can doubt : and it appears to me to be the more probable that all the heat which a thermometer placed below the surface of warm water acquires when a great degree of heat is applied to the particles of that fluid which are at the surface, is in fact received from the sides of the containing vessel, not only because the thermometer acquires heat so very slowly, but also, and more especially, because this heat is acquired much more slowly when the containing vessel is wide than when it is narrow ; and also when it is made of a substance which is a good conductor of heat than when it is constructed of a substance which is a bad conductor of heat, as I have found by experiment. But as this particular enquiry is foreign to my present purpose, I shall not enlarge on it in this place,

Facts which shew the probability that considerable heats are chiefly transmitted downwards through fluids by the agency of the vessel.

## II.

*Description of a Press for preserving botanical Subjects; with an Account of the Success of the Improvement in the Art of Blasting pointed out by Mr. JESSOP in this Journal. In a Letter from Mr. THO. HARRISON.*

To Mr. NICHOLSON.

SIR,

Introductory  
obs. on preserv-  
ing plants.

BEING desirous to make a collection of the plants which grow in the neighbourhood of Kendal, and to preserve them in an herbarium, I was naturally led to a consideration of the different methods, which have been used by botanists, of drying and preserving specimens; but finding none so complete and expeditious as I could wish, I therefore adopted a plan of my own, which I am persuaded will be found to answer every purpose that the botanist can require. The real utility which is to be derived by a young botanist from the preservation of plants, is the power of future examination, and to answer the purpose the natural appearance of each specimen ought to be preserved as calculated to produce this effect; for since the seed-vessels and stems of plants occupy much more space in thickness than the leaves, in order to preserve the latter from shrivelling, the two former must generally be bruised in such a manner as totally to preclude any accurate investigation afterwards: and this on trial is found to be the case: besides, all the plants that happen to be in the press at the same time, however various their texture, are subjected to the same degree of pressure.

Remarks on the  
methods now in  
use.

The plan recommended by T. Velley, Esq. seems equally exceptionable; I mean the method of placing the plant when fresh between several sheets of blotting-paper, and ironing it with a large smooth heater pretty strongly warmed, till all the moisture is dissipated. By this method it is evident that the parts of fructification must be much bruised: it may preserve the colour of the blossoms better than any other, but this in the science of botany is not very essential, at least if it was, I believe that no method hitherto discovered will succeed universally in arresting the fading beauty of delicate flowers. The next plan which is given by Dr. Withering, is one used  
by

by a Mr. Whateley a surgeon, but his contrivance, though certainly better adapted to preserve the parts of fructification and the shape of the stem entire than the two former, I found would take up much more time than a country surgeon can well spare; therefore this led me to make what I thought an improvement on Mr. Whateley's plan, and the experienced botanist is left to judge of my success from the following description; this I can say for it, that it answers completely (of course) to my own wish; but further, that Mr. John Gough, of Middleshaw, who has the best collection of plants in this county, regrets that he had not the same contrivance, and has urged me much to send you the following description of my press.

This instrument consists of 17 oblong boxes, each, except-  
 ing the uppermost and lowermost, is made of four sides of well-seasoned oak wood, two inches deep and one-fourth of an inch thick, dove-tailed together; the two end-pieces of which have two notches each in the middle wherein to place the fingers in lifting it; and the bottom consists of canvas glued and nailed to the wooden frame: in each corner is fixed a small triangular piece of wood reaching half an inch from the bottom upwards (for a purpose hereafter to be explained). These boxes are made so as to be placed one within another successively upwards, the lowest (I am speaking only of the 15 with canvas bottoms) measures 20 inches by 16 on the outside, and the highest 12 inches by 8. The bottomest, or carriage on which all the others are supported, is much stronger, the sides of it being two inches deep and three quarters of an inch thick: its bottom is of wood of the same thickness, all over perforated with holes one-third of an inch in diameter, and rests on four iron castors, one at each corner, to render the whole more easily moveable; at each end of it there is an iron handle: This box is 20 inches by 16 within, and will therefore barely receive the largest of the boxes with canvas bottoms. The uppermost, or 17th box, is of the same construction as the canvas boxes, excepting that its bottom is of wood of the same thickness as its sides, with a number of holes pierced through of the same magnitude as those in the carriage, and canvas glued to the under side of it. To complete this press, two folds of blotting-paper are to be placed

The author's new press described. It consists of a series of trays or boxes, having (all but the lowest) canvas bottoms. They contain sand, and the plants are laid between box and box.



placed over the holes in the bottom of the carriage, in which, as well as all the canvas-boxes, must be put an inch in depth of the finest sand, washed and sifted; river sand is most eligible, as being least angular; which sand must be made level on the top by drawing over it a rule that reaches one inch into each box: these boxes are placed one within another, and each by containing an inch depth of sand, raises the incumbent frame one inch, so that when they are thus charged and placed one upon another, the whole makes a truncated pyramid of 18 inches in height, and weighing about 1 cwt. The uppermost box may be filled with sand, or, if more weight be necessary, with shot. To this box I have added (what is by no means essential) a cap, consisting of the sides of three boxes fixed one within another, each projecting one inch above its inferior, for the sake of uniformity; the top is covered by a thin board: when this cap is placed upon the uppermost box, the whole has not an inelegant appearance.

Reference to the engraving.

Left the description should not be sufficiently clear, I have sketched the following figure, representing a perpendicular section of the press viewed on one side of it. *Plate XV.*

*Fig. 1.* represents a perpendicular section of the press made lengthwise.

- a.* The end of the bottomest box or carriage.
- b.* Its bottom part perforated with holes.
- c.* The end of the first box with a canvas bottom.
- d.* The canvas bottom.
- e.* The sand which but half fills the box.
- f.* The 17th or uppermost box, with a perforated wooden bottom.

*g g g.* Represent the end of the cap formed of the frames of three boxes, in the empty space of which may be placed the botanist's pocket box.

*Fig. 2.* represents the rule wherewith the sand is made level in the boxes; it is 18 inches long.

- h.* A notch one inch deep.
- i.* The edge made thin.
- k.* A notch an inch long.

*Fig. 3.* represents the end of a canvas box.

*ll.* The two notches to place the fingers in when the box is to be lifted.

*The*

*The Method of using it.*

After a specimen is selected, a slip of paper is to be attached to it, containing its name, place of growth, and time of gathering. The boxes are then to be taken off by three or four at a time, until the number removed may be judged to be of sufficient weight to produce the necessary pressure. On the sand in the box thus selected for use, a single fold of blotting-paper must be placed; upon that the plant, taking care to preserve as much as possible the natural position of its characteristic parts: Over the plant a second fold of paper must be laid, and then the boxes are to be replaced. If the plant be woody and require much pressure, place it near the bottom; but if succulent, near the top; and herein consists one great advantage of this press over every other, since the pressure can be selected or varied at pleasure, and all the parts of the plant pressed equally; for if the plant be succulent, the pressure can scarcely be too slight at first, and may be gradually increased without ever bruising the tenderest part; and if, on the other hand, strong pressure be required, the plant may be placed in the bottomest box; and if the weight of sand be not thought sufficient, any number of the boxes next above it may be loaden with small shot, by which the pressure may be increased until the boxes be full, when the pressure exerted would be much greater than the strongest plants could bear without having some of its essential parts defaced. Another advantage which led me to give a preference to this method of pressing plants, was the great saving of time; for though my professional avocations should require my immediate presence at a distance, and thus drag me from this innocent amusement when in the busiest part of classing and arranging my specimens, and though my press should be completely separated into its constituent parts, yet the whole can be arranged, in the space of one minute, to remain until the next leisure time that may occur; so that, in fact, with this contrivance, botany is a study that can be entered into or given up as it may suit the convenience of a moment; which is not the case where any of the plans described in Dr. Withering's Introduction to the Study of Botany \* are followed.

Method of using it.  
The plants are laid between blotting-paper, and the pressure regulated by the number of boxes above each, &c.

Remarks on the effects and advantages of this contrivance.

It may be necessary to explain some parts of the structure of the press. The holes in the bottomest and uppermost boxes are to admit of the circulation of air and the evaporation of moisture, which will be considerable when the press without its cap is placed within the influence of the sun or fire, as it always ought to be: and the small triangular pieces of wood in the corners of each box, are to prevent the upper boxes from pressing solely on the canvas bottom of the lowest box, when any number are lifted together: these may at first sight appear likely to impede the pressure of the sand when placed in the press, but this objection will vanish when it is recollected that each box contains an inch depth of sand: and it is evident, on examination of the press, that the sides of the boxes serve no other purpose than to keep the sand together; for they are perceptibly moveable in a perpendicular direction, and sustain no pressure from the superior boxes; therefore the plant is placed as if in a heap of sand.

The best method of preserving dried plants is to stitch them to a paper.

I am afraid I have already trespassed too long on the attention of the reader, but I cannot dismiss the subject without offering a few observations on the manner of preserving plants after they are dried. There have been at least three methods used: first, fixing them to paper by the aid of gum-water: second, placing them loosely in a book: third, stitching them with thread to a sheet of paper. The first plan appears to me the least eligible, for it cannot be neatly executed unless the plant be pressed nearly flat, and by fixing it to the paper it can never admit of an accurate examination in future, but becomes a mere picture, and a very imperfect one; for almost every distinguishing character, especially the parts of fructification, are injured or defaced; even the general habit is often so much altered that the plant could not be ascertained, were it not for the name generally subscribed at its root. The second plan is also objectionable; for though the plant may at any time be subjected to examination, yet by lying loose in a book, it is very apt to be broken by the least motion, particularly when kept dry, which all preserved plants ought to be. The third appears to me the least liable to objection; for if the plant be neatly stitched down, it will not be subject to such motion as to injure its parts; and by cutting the threads



threads it may be detached from the paper, and subjected to any future investigation.

I am, Sir,

Your's, &c.

Kendal, July 8,

THOMAS HARRISON.

1805.

P. S. I cannot refrain from calling to the recollection of your readers a valuable paper in the 9th Vol. p. 230, of your Journal, recommending the use of sand in the blasting of rocks. The effects there related of this simple agent, I confess did astonish me more than any thing I ever read: and as very serious and even fatal accidents have happened by the method of ramming blasts with stone in the limestone and slate quarries in this neighbourhood, I was determined to repeat the experiment the first opportunity, and thus shew its effects to the men who work in those places. Accordingly no long period elapsed before I had occasion to be in the neighbourhood of the slate quarries in Longliddale, about ten miles from Kendal; when I went to one of those quarries, taking with me a bag of small sand, consisting of powdered freestone used in this country to scour pans with, and strew upon the stone floors. I mentioned my business to one of the workmen who was then engaged in boring a hole 24 inches deep and about an inch diameter in a slate rock; the stratum was about 27 inches thick, and reclined from the perpendicular about 20 degrees; and this being the first blast, it was consequently firmly surrounded and fixed in on all sides by solid rock: The part that he wanted to throw out by the blast was supposed to be five ton weight: The direction of the hole which he was boring was perpendicular to the stratum, and therefore elevated above the horizon about 20 degrees. He smiled at my proposal, but said he would try the experiment to satisfy me, provided I would pay for the powder if it failed; but he seemed to think that I had a very poor idea of blasting, to believe that a little light sand would answer the purpose of the laborious and dangerous process of ramming which he had been accustomed to use. However, he charged his blast with powder in the manner he had been used to do; i. e. into this hole

Account of the successful application of the method of blasting rocks indicated in this Journal by W. Jessop, Esq.

Safe and easy  
method of blast-  
ing rocks.

of 24 inches in depth, he put as much powder, together with two or three pieces of broken sticks, of half an inch diameter and about four or five inches long, as reached 12 inches upwards: The sticks he introduced for the purpose of keeping the powder loose in the hole, so that it might take fire more nearly all at the same instant: This done, he put in a charged straw, into the outer end of which was fixed a match-paper; and lastly, he (with a smirking countenance, anticipating the joy he expected to feel at my disappointment) filled up the hole with sand. The match was then fired, upon which we removed to a distance, and the powder soon exploded; when he immediately exclaimed, from the peculiarity of the report, though the effect was unseen by him, that the rock was as completely shattered as if the hole had been rammed with stone; and a view of the place verified the prediction. Since that time the plan has been adopted in that and most of the neighbouring quarries, with the use of small sand which is washed out of the river in floods, and the method answers as well as the old one which was attended with so much danger. In one case the sand was thrown out twice without any effect on the rock; the third time, the hole was rammed in the usual manner with stone, which was thrown out likewise; so that the new process fortunately lost no reputation.

I have detailed thus much in confirmation of the valuable paper before mentioned, and in hopes that it may induce some of your readers who reside in the neighbourhood of mines or quarries, to endeavour to introduce this safe and easy method in lieu of the dangerous and tedious one of ramming with stone, in which process we have had repeated instances of the loss of limb, vision, and even life itself: In the use of sand there is comparatively no danger, and by adopting this plan many a valuable life will be saved.

*Description*

## III.

*Description of a Safety Valve, containing a Vacuum Valve in the same Hole of the Boiler. By Sir A. N. EDELCRANTZ.\**

IN large boilers or coppers, where boiling fluids are enclosed, a *safety valve* is generally used to prevent their bursting, from an unexpected excessive force of the elastic steam, and, besides, a *vacuum valve*, to prevent their being compressed or crushed by the weight of external air, in the case of a sudden condensation of the vapours. These two valves are commonly fitted in two different holes in the boiler; but as a more simple, and consequently more eligible, method seems to be that of joining them together, I take the liberty to submit to the Society for the Encouragement of Arts, &c. the following contrivance for that purpose:—

*a b*, Plate XII. Fig. 2. is a common conical safety valve, fixed in the boiler *c d*, having four openings, *i i*, which are represented in a plan view in Fig 3: *e f* is the metallic rod, bearing the weight *K K*, with which the safety valve is loaded, and extending itself under that valve to *f*: *g h* is the vacuum valve consisting in a plane circular plate, with a brass tube sliding along the rod, and pressed by a spiral spring to the safety valve *a b* (against which it has been well ground in making it), closing in that situation the openings *i i*.

Such being the construction of the whole, it is evident, that when the elasticity of the steam increases, the two valves, joined together, with the holes *i i* shut, make but one, opposing to the elasticity of the steam an united resistance, which is regulated by the weight *k k*, in the common way; but, on the contrary, when by condensation of the vapours a vacuum is produced, the external air in pressing through *i i*, upon the vacuum valve *g h*, forces it down, and opens to itself a passage into the boiler.

The valve *g h* may easily be made conical, like the other, if that form should be preferred; but in different trials, I have found planes, if well turned and ground together, join as per-

\* From the Memoirs of the Society of Arts for 1804. The silver medal was awarded for this invention.



fectly as can be desired, being pressed by the united elasticity of the spring and the steam. †

The same applied to a former contrivance.

Fig. 4. is the same contrivance adapted to a new kind of *safety valve* or *piston*, which, though I originally intended it for the use of Papin's digesters of a new construction \*, has been, in a larger size, applied by me even to steam engines, and is described in the Philosophical Magazine of December, 1803. †

Other experiments.

I have lately begun, and shall pursue, a set of experiments, with the intention of regulating by this safety piston, the quantity of admitted air to fire-grates, and to effect, by that means, a new mode of regulating the fire, and the elasticity of steam in boilers, with less expenditure of fuel and of force than usual; of which idea a hint is given in the work and place above mentioned. The result of these researches I shall at some future period do myself the honour of communicating to the Society.

#### IV.

*Observations and Experiments for the Purpose of ascertaining the definite Characters of the primary Animal Fluids, and to indicate their presence by accurate chemical Tests. By JOHN BOSTOCK, M. D. Communicated by the Author.*

Great want of precision in the animal analysis; particularly of the fluids.

THE precision which the analysis of mineral and vegetable substances has attained, does not appear to be yet extended to

\* Nicholson's Journal, March, 1804.

† The description of this contrivance being already published, it would be superfluous to repeat it. I only beg leave to add the following practical remark. A metallic piston, if well turned and fitted into a cylinder of exactly the same kind of metal, will probably have the same degree of expansion, especially if hollow, and consequently will not increase its friction in any increased degree of temperature. But as in practice, the cylinder is commonly exposed to a lower temperature than the piston, heated by the steam, a little increase of friction will take place by an increase of heat. To prevent the effect of this, I have found it useful to employ for the piston a metal of somewhat less expansive power than the cylinder: and the expansion of red copper being to that of brass nearly as 10 to 11, I prefer making the piston of the former metal, when the cylinder is made of brass.

the

the products of the animal kingdom. This remark may be applied both to the solids and fluids which compose the animal body, but it is the most applicable to the latter class of substances. The terms serous, mucilaginous, gelatinous, &c. are employed, even by the most esteemed medical and physiological writers \*, in a vague and indeterminate manner, without attending either to the original import of the word, or to the restricted meaning, which it is necessary to impose upon popular expressions when they are adopted in scientific researches. The object of the present paper is to ascertain a definite character for what I propose to call the primary animal fluids, and to discover delicate and accurate tests, by which their presence may be easily and certainly indicated. By primary animal fluids, † I mean those into which the compound fluids existing in the animal body are capable of being resolved by the application of different re-agents, without decomposing them into their ultimate elements.

Primary animal fluids.

#### *Albumen.*

The first of the animal fluids which I propose to make the subject of my investigation is the albumen. With the exception of water, no fluid appears to enter so largely into the composition of the animal body. It forms a very considerable proportion of the blood, and is found in greater or less quantity in nearly all the secretions. It is also capable of assuming the solid form, without undergoing any change in its chemical properties; in this state it constitutes the basis of all the membranous substances, which are so extensively dispersed through every part of the system; it composes the cellular tissue into which the earth of the bones, and the fibrous matter of the muscles are deposited, while it enters largely into the structure of the skin, the glands and the vessels. At present, however, we shall direct our attention to it while in the liquid form.

Albumen, a large component part of animals.

\* Even Mr. Abernethy, in his late valuable work on tumors, speaks of the gelatinous part of the blood, where I conceive from the context, there can be no doubt that he intended to designate the fibrine.

† All the animal fluids, both primary and compound, are merely solutions of a solid body in water; but those substances which are most frequently seen in a state of solution, have very generally obtained the title of fluids.

In

Most conveniently obtained from white of egg.

White of egg is not pure albumen; it contains a little of matter not coagulable.

White of egg contains 80 parts water; 15.5 solid albumen and 4.5 uncoagulable matter.

In order to obtain it in a state of purity, I had recourse to the white of the egg, a substance to which the name of albumen was originally applied, and which is still considered by the most eminent chemists \* as composed entirely of this substance. In order to ascertain how far this opinion was correct, I kept a quantity of the white of the egg in a temperature of  $212^{\circ}$ , until it was firmly coagulated. It was then cut into small pieces and placed in the upper part of a narrow-necked funnel, when a few drops of a brownish viscid fluid were separated from it. Other pieces of the same coagulum were kept for some time in boiling water, the fluid being then passed through a filtre, had acquired a light brownish colour, and a faint odour; when agitated it was slightly mucilaginous. By slow evaporation, a small quantity of a brittle, semi-transparent substance was obtained. It appeared therefore evident, that the white of the egg contains a small quantity of a substance incapable of coagulation, and therefore essentially different from albumen. I still, however, continued to employ it for the purpose of ascertaining the properties of albumen, as it affords this substance in a state of greater purity than it can be obtained from any other source.

It was an object of some importance to ascertain the proportion which this foreign ingredient bore to the albumen itself; 100 grains of firmly coagulated albumen were kept for some time in boiling water, and this being poured off, a fresh quantity was added, and this process repeated until the water appeared to contain no farther impregnation. The whole of the fluid was then evaporated, and a residuum was left which amounted to  $4\frac{1}{2}$  grains. Besides the admixture of this peculiar substance, the white of the egg contains a considerable quantity of water, not only in its liquid state, but after it is coagulated. By a gentle heat the water may be evaporated, and the solid matter is left behind in the form of a hard, brittle, transparent substance; I have found that upon an average,  $\frac{4}{5}$  of recently coagulated albumen may be considered as consisting of water. Hence it will appear, that 100 grains of the white of egg consist of 80 grains water, 4.5 grains of uncoagulable matter, and 15.5 grains only of pure albumen.

\* Hatchett, Phil. Transf. 1800, p. 375.  
Thomson's Chemistry, IV. p. 484.



The most distinguishing characteristic of albumen is the property of being coagulated by heat, which forms an obvious and easy test of its presence, when it exists in a compound animal fluid in any considerable proportion. In order to ascertain how small a quantity of albumen could by this means be rendered visible, I added 13 grains of the white of the egg to 87 grains of water, and thus formed a solution, one grain of which contained  $\frac{1}{30}$  grain of pure albumen. Five grains of this solution were then added to 95 grains of water, so that 100 grains of water contained  $\frac{1}{10}$  grain only, or  $\frac{1}{1000}$  of its weight of pure albumen. This was exposed to the heat of boiling water, and a perceptible opacity was produced in the fluid.

The effects of the oxymuriate of mercury were next tried. One drop of a saturated solution of this salt, added to 100 grains of water, containing  $\frac{1}{1000}$  of its weight of albumen, produced a very evident milkiness; after some hours a curdy precipitate separated and fell to the bottom. A solution of half the strength, containing only  $\frac{1}{2000}$  of its weight of albumen, was then tried by the same re-agent, and even in this instance, a sufficiently obvious effect was produced.

The nitro-muriate of tin is a powerful coagulator of albumen in its unmixed state, but I found it not to be so delicate a test as the oxymuriate of mercury. One hundred grains of water, containing  $\frac{1}{2}$  grain of albumen, *i. e.*  $\frac{1}{200}$  of its weight, was not affected by this re-agent until after some hours, when the fluid exhibited a degree of milkiness.

In order to ascertain the effects of tannin upon albumen, I macerated half an ounce of powdered galls in half a pint of water for some hours, and filtered the fluid. A deep brown-transparent liquor was produced, 100 grains of which I found by evaporation to contain  $2\frac{1}{2}$  grains of solid residuum. Equal parts of this preparation of galls and of a solution of albumen, in the proportion of one part to 1000 parts of water, were mixed together; at first no effect could be perceived, but after some time an evident precipitate was formed and slowly subsided.

The aqua lithargyri acetati, or extract of Goulard, is an active precipitant of several animal fluids; when dropped into a strong solution of albumen, a very copious and dense precipitate is immediately formed.

It

—but doubt-  
fully.

Aqua lithargyri  
acetati plentiful-  
ly precipitates it.

It is, however, somewhat difficult to determine how far this effect depends upon the albumen itself, as Goulard has been considered to be the appropriate test of the uncongelable part of the serum of the blood, which, it may be supposed, resembles the uncoagulable part of the white of the egg. The aqua lithargyri acetati is likewise decomposed by several of the saline bodies which are found to exist in almost all the animal fluids. I have sometimes found it to yield a precipitate even when added to distilled water, and in all cases, after exposure to the atmosphere for a few hours, the water to which it has been added becomes turbid, and is covered with a thin film. In order to try the effect of this re-agent on albumen, I added one drop of it to 200 grains of water, and upon observing that the transparency of the fluid was not affected, a single drop of the solution of albumen, of the same strength with that mentioned above, was added. It formed a dense precipitate as it fell through the fluid, and upon agitation, the whole was rendered slightly milky. In this case the proportion of the water to the albumen was as 10,000 to 1; to the uncoagulable part of the white of the egg, it would be about as 30,000 to 1.

Nitrate of silver  
also has a like  
effect;

The next re-agent which I employed was the nitrate of silver. A single grain of a saturated solution of this salt produced an evident turbidness in 100 grains of water, containing  $\frac{1}{10}$  gr. of albumen, and after some hours a small quantity of a curdy precipitate fell to the bottom of the vessel. It might, however, be suspected that in this case, the effect produced depended upon a quantity of muriate of soda contained in the albumen. I found that 100 grains of water, containing only  $\frac{1}{200,000}$  gr. of common salt was rendered evidently turbid by one drop of the nitrate of silver; \* but the precipitate which

\* I weighed very exactly a grain of salt, and dissolved it in 200 grains of water. One grain of this solution was afterwards added to 99 grains of water, and by repeating the operation for three successive times, I obtained 100 grains of water, containing only  $\frac{1}{20,000}$  of its weight of salt. I then took 99 grains of distilled water, and poured into it one drop of the nitrate of silver: after waiting for some time, until I was satisfied that no effect would take place, I added a single drop of the last solution of salt, thus making it  $\frac{1}{200,000}$  part of the mixture; a faint but perceptible opacity was almost immediately produced.



is formed by the nitrate of silver acting on the muriate of soda is in the form of a greyish powder, and subsides more rapidly than it did in the former instance, where it produced a white, flaky precipitate.

I found that a solution of albumen of the same strength with —and nitro-muriate of gold; that employed in the last experiment, was immediately decomposed by the nitro-muriate of gold. One drop of this metallic solution instantly produced a dense white precipitate in 100 drops of water, containing only  $\frac{1}{10}$  of a grain of albumen.

Albumen in a concentrated state is powerfully coagulated —and alum. by alum; I found however, that this re-agent is not so accurate a test of its presence when in a diluted state, as some of those which I had already employed:  $\frac{1}{5}$  grain of albumen, dissolved in 100 grains of water, was indeed rendered slightly turbid by the addition of a few drops of a saturated solution of alum, but no precipitate was formed.

Before I conclude my account of these experiments I must observe, that the strength of the solution of albumen was in all cases rather less than my estimate. When I added the albumen to the water, a small portion of it always remained insoluble, and this was separated from the fluid by filtration before the experiments were performed. This insoluble part I supposed to consist of the membranous matter, with which it is said that the white of the egg is intersected. The quantity was indeed almost too small to be appreciated, but where it is desirable to attain as much accuracy as possible, I think it necessary to mention every circumstance which may in the smallest degree affect the result. Obs. on the solution of albumen.

The experiments related above will, I conceive, indicate with a sufficient degree of accuracy, the presence of albumen as a constituent of an animal fluid. The property of being coagulated by heat is a characteristic of this substance, which will always serve as a mark of discrimination, and we have found that this property is not destroyed by dilution with 1000 times its weight of water. This therefore may be considered as a test of its presence minute enough for all practical purposes. We have also found that there are several re-agents which possess the power of precipitating it from its solution in water, while existing only in the same proportion. It will be necessary, however, to observe their operation upon the other animal substances before we can determine their use in the analysis or compound fluids. Remarks. The coagulation by heat is a good distinctive character of albumen.

*Jelly.*



*Jelly.*

Jelly. It is liquifiable by heat and becomes concrete by cold; and is a large component part of animals.

The next substance which I propose to examine is jelly. The peculiar characteristic of this body is its property of becoming concrete by cold, and being liquified by the application of a gentle heat. It enters into the composition of the blood, though less largely than the albumen. It is also an ingredient in the skin, membranous texture, ligaments, cartilages and tendons. By boiling it is easily extracted from these substances, and by evaporation and cooling the whole is reduced to a mass of greater or less solidity, in proportion to the previous degree of concentration. By a process of this nature, isinglass is prepared from the bones and cartilages of fish; as this substance has been considered to consist of jelly nearly in a state of purity, I employed it for the following experiments.

Obtained by the solution of isinglass. One fiftieth part in water will coagulate.

Four grains of isinglass were dissolved in 200 grains of water, and thus a standard fluid was formed, one grain of which contained  $\frac{1}{50}$  gr. of jelly. This solution became perfectly concrete by cooling. In the first place I wished to ascertain how small a proportion of jelly dissolved in water was capable of assuming the concrete state. Equal parts of the standard fluid and of water, *i. e.* one part of jelly to 100 parts of water, produced a compound which was completely stiffened by cooling; but I found that two parts of water to one of the standard, *i. e.* one part of jelly to 150 parts of water, produced a compound, which though evidently gelatinous, did not assume the concrete form.

It is actively precipitated by tania.

One of the most active precipitants of jelly is the tanning principle. I found that a mixture of 5 grains of the standard solution and 95 of water, produced a copious precipitate when added to an equal quantity of an infusion of galls of the same strength with that employed in the experiments upon albumen. In this instance the jelly was to the water as 1 to 1000. Afterwards reduced the quantity of jelly until it composed  $\frac{1}{3000}$  part only of the solution, and in this case a considerable precipitate was still produced by the infusion of galls.

—but not by aq. lith. acet.

A quantity of the standard solution had a few drops of the aqua lithargyri acetati added to it, but no more effect appeared to be produced than would have ensued from mixing it with an equal quantity of pure water.

No effect was produced by adding the oxymuriate of mercury to the standard solution. The nitrate of silver and the nitro-muriate of tin were each employed, and produced a very slight and almost imperceptible opacity.

The addition of the nitro-muriate of gold caused a small quantity of a dense precipitate when added to the standard solution; but when this was so far diluted as to contain one grain of jelly in 500 of water, the effect was no longer perceptible.

### *Mucus.*

Animal mucus or mucilage enters largely into the constitution of many parts of the body, and forms a considerable proportion of several of the secretions. The term mucus had been generally employed in a vague and unrestricted sense, until Mr. Hatchett, in his valuable paper on the membranous parts of animals, inserted in the Phil. Transf. for 1800, attempted to assign to it a more appropriate and definite meaning. He conceives that jelly and mucus are only modifications of the same substance, and do not essentially differ from each other; he considers it to be entitled to the appellation of mucus, when it is soluble in cold water and cannot be brought to the gelatinous state\*. Dr. Thomson adopts in general the idea of Mr. Hatchett, and lays down the following as the characteristic properties of animal mucilage. It is soluble in cold water, insoluble in alcohol, neither coagulable by heat nor generating into a jelly, precipitable by tan and the nitro-muriate of tin†. I have been induced from the results of my observations, to form a different opinion respecting the relation of jelly and mucus, but I shall defer the statement of it until I have related the experiments which have led me to dissent from such high authority.

By agitating for a short time some recent saliva in cold water, part of it was dissolved, and after being passed through a filter, was made the subject of experiment, being, as I conceived, a solution of nearly pure mucus. By a careful evaporation I found that the water had dissolved  $\frac{1}{248}$  part of its weight.

\* Hatchett, Phil. Transf. 1800. 369 and 381.

† Thomson, IV. 503.

Effect of re-  
agents. No co-  
agulation by  
moderate heat,  
nor did it gelati-  
nise by cooling.

No effect was produced by the addition of the oxymuriate of mercury, and the nitro-muriate of tin caused only a slight opacity. No effect was produced by the addition of equal parts of this solution and the infusion of galls. The aqua lithargyri acetati added to the solution produced an immediate opacity, and after some time a white, fleaky precipitate fell to the bottom of the glass. No appearance of coagulation was produced by exposing the fluid for some time to the heat of boiling water, nor was there any tendency to gelatinize by evaporating and afterwards cooling the fluid.

Mucus from an  
oyster.

I afterwards endeavoured to obtain mucus in a state of purity from another source. For this purpose an oyster was agitated for a few minutes in cold water; the fluid was filtered and appeared slightly opake and glutinous.

By evaporation it appeared that it had dissolved about  $\frac{1}{30}$  of its weight of animal matter. A quantity of this solution, diluted with an equal bulk of water, was employed in the following experiments.

Reagents.

The oxymuriate of mercury being added to it produced no effect. The infusion of galls after some time produced a slight degree of turbidness, and at length a precipitate was formed in small quantity. The aqua lithargyri acetati caused an immediate opacity, and after some time a dense precipitate.

The Goulard in-  
dicates its pre-  
sence.

These experiments nearly coincide with the former. In both cases no effect was produced by the oxymuriate of mercury, thus proving the absence of albumen. The small precipitate caused by the galls indicate the existence of only a very minute quantity of jelly. The effect was scarcely as great in this instance, where the animal matter composed  $\frac{1}{100}$  part of the weight of the solution, as was produced by the same reagent upon a solution of jelly, where it composed only  $\frac{1}{3000}$  part of the weight of the fluid. Very nearly the whole therefore of the animal matter probably consisted of mucus, the presence of which was indicated by the Goulard.

Exclusive tests;  
tan for jelly,  
aq. lith. acet.  
for mucus, ox.  
m. of merc. for  
albumen.

I apprehend that these experiments will be deemed sufficient to establish a decided and essential difference between mucus and jelly, independent of the gelatinizing property of the latter, the effects produced upon them by the tanning principle and by the aqua lithargyri acetati are exactly opposite. Tan is a most delicate test of jelly, but does not in any degree affect mucus. Aqua lithargyri acetati is a delicate test of mu-  
cus



tus, but does not in any degree affect jelly. The oxymuriate of mercury, on the contrary, which is one of the most accurate tests of albumen, does not appear to be affected either by jelly or by mucus.

Albumen, jelly, and mucus, I am inclined to consider as the only primary fluids which are dispersed through the different parts of the body. Particular vessels or glands contain and secrete particular fluids, which cannot be resolved into other fluids without decomposition, as the fibrine of the blood, the resin of the bile, the urée of the kidney, &c. but these are in all instances confined to their appropriate organs, and do not necessarily enter into the present investigation.

From the above experiments I think we may be entitled to lay down, with a considerable degree of accuracy, the leading characteristics of the three primary animal fluids, and to establish tests by which their presence may be minutely ascertained. The most remarkable property of albumen is its becoming coagulated by heat, a property which it retains so far as to communicate a degree of opacity to its solution in water, when it forms only  $\frac{1}{1000}$  part of its weight. A solution of the same strength has its albumen precipitated by the oxymuriate of mercury, and this test will indicate its presence when composing no more than  $\frac{1}{2000}$  of the mixture. The tanning principle, the aqua lithargyri acetati, the nitrate of silver, and the nitro-muriate of gold, are all tests of the presence of albumen nearly as minute as the oxymuriate of mercury, but they are less valuable, because their effects are not confined to albumen. The nitro-muriate of tin and alum are also precipitants of albumen, but they are less delicate in their operation than the reagents enumerated above.

The peculiar characteristic of jelly is its property of becoming concrete by cold, and being again rendered fluid by a gentle heat: we have found that its solution in water retains this property when it composes  $\frac{1}{100}$  part only of the weight of the fluid. Tan is a still more minute test of jelly than of albumen, but jelly is not in the least degree affected by the oxymuriate of mercury, and may thus in all cases be easily distinguished from it. No effect is produced in jelly by Goulard, and scarcely any by the nitrate of silver, and the nitro-muriate of tin, when it is in a state of such dilution. By means of tan, jelly may be easily detected in a fluid of which it forms only  $\frac{1}{1000}$  part.

These are the primary and general fluids.

Resumption.

Albumen is known by its coagulability, and precip. by ox. m. merc.

Jelly is known by its concretion on cooling and its precip. by tan.

Mucus is negative as to the preceding characters; but precip. by aq. lith. acet.

The properties of mucus are principally negative; it is not coagulable by heat, nor capable of becoming gelatinized; it is not precipitable either by the oxymuriate of mercury or by tan, but it may be detected with considerable minuteness by the aqua lithargyri acetati.

#### Other tests.

It appears therefore that the oxymuriate of mercury, tan, and the aqua lithargyri acetati are the three most valuable tests. The nitro-muriate of tin is a less delicate test of albumen than the oxymuriate of mercury, and is also in some degree affected by jelly. The nitrate of silver appears to be a very nice test of albumen, but it is objectionable in consequence of its being decomposed by the muriate of soda, a salt which is supposed to exist in most of the animal fluids. The nitro-muriate of gold is a delicate test of albumen, but it likewise precipitates jelly.

#### Order of analysis.

In the analysis of a fluid which is supposed to contain either albumen, jelly, or mucus, the first step is to observe the effect of the oxymuriate of mercury; if this produce no precipitate, we may be certain that the fluid in question contains no albumen. We must next employ the infusion of galls, and if this also cause no precipitate, we may conclude that the animal matter held in solution consists of mucus alone.

#### Remark.

I have before remarked, that the ideas which I have formed of the nature of jelly and mucus, and the relation which these substances bear to each other, differ materially from those of Mr. Hatchett. It is not indeed without a degree of diffidence that I dissent from so distinguished a chemist; but I conceive that I am justified by the experiments related in this essay.—Mr. Hatchett, in the valuable paper to which I have already referred, speaks of the white of the egg as consisting of pure albumen; but I believe that in this particular he will be found not perfectly accurate.

Animal mucus resembles veget. gum.

There is a great resemblance between the mechanical properties of animal mucus and vegetable gum, and I found that they strongly resemble each other also in their chemical qualities. A solution of gum arabic, containing one grain of gum to 200 grains of water, was not affected either by the oxymuriate of mercury or by tan. With the nitro-muriate of tin and with the nitrate of silver there was only a slight degree of opacity, but with the aqua lithargyri acetati there was a dense precipitate instantly formed.



## V.

*On muscular Motion.* By ANTHONY CARLISLE, Esq. F. R. S.  
being the Croonian Lecture, read before the Royal Society,  
November 8, 1804.

(Concluded from Page 201.)

THE loss of motion and sensation from the influence of low temperature, accompany each other, and the capillaries of the vascular system appear to become contracted by the loss of animal heat; as in the examples of numbness from cold. Whether the cessation of muscular action be owing to the impeded influence of the nerves, or to the lowered temperature of the muscles themselves, is doubtful; but the known influence of cold upon the sensorial system, rather favours the supposition that a certain temperature is necessary for the transmission of nervous influence, as well as sensation.

Cold destroys  
mobility and  
sensation.

The hybernating animals require a longer time in drowning than others. A full grown hedge-hog was submersed in water at 48°, and firmly retained there; air-bubbles began instantly to ascend, and continued during four minutes; the animal was not yet anxious for its liberty. After seven minutes it began to look about, attempting to escape; at ten minutes it rolled itself up, only protruding the snout, which was hastily retracted on being touched with the finger, and even the approach of the finger caused it to retract. After fifteen minutes complete submersion, the animal still remained rolled up, and withdrew its nose on being touched. After remaining thirty minutes under water, the animal was laid upon flannel, in an atmosphere of 62°, with its head inclined downwards; it soon began to relax the sphincter muscle which contracts the skin, slow respirations commenced, and it recovered entirely, without artificial aid, after two hours. Another hedge-hog submersed in water at 94°, remained quiet until after five minutes; about the eighth minute it stretched itself out, and expired at the tenth. It remained relaxed, and extended, after the cessation of the vital functions; and its muscles were relaxed, contrary to those of the animal drowned in the colder water.

Hybernating  
animals not easily  
drowned.  
Experiment with  
the hedge-hog.

The irritability of the heart is inseparably connected with respiration. Whenever the inhaled gas differs in its properties  
from

Connection of  
irritability with  
respiration, &c.



from the common atmosphere, the muscular and sensible parts of the system exhibit the change: the actions of the heart are altered or suspended, and the whole muscular and sensorial systems partake of the disorder: the temperature of animals, as before intimated, seems altogether dependant on the respiratory functions, although it still remains uncertain in what manner this is effected.

Distribution of heat by the blood.

The blood appears to be the medium of conveying heat to the different parts of the body; and the changes of animal temperature in the same individual at various times, or in its several parts, are always connected with the degree of rapidity of the circulation. It is no very wide stretch of physiological deduction to infer, that this increased temperature is produced by the more frequent exposure of the mass of blood to the respiratory influence, and the short time allowed in each circuit for the loss of the acquired heat.

Coagulation of that fluid.

The blood of an animal is usually coagulated immediately after death, and the muscles are contracted; but, in some peculiar modes of death, neither the one, nor the other of these effects are produced: with such exceptions, the two phenomena are concomitant.

Heat delays the last coagulation and the contractions of the muscles, &c.

A preternatural increase of animal heat delays the coagulation of the blood, and the last contractions of the muscles: these contractions gradually disappear, before any changes from putrefaction are manifested; but the cup in the coagulum of blood does not relax in the same manner; hence it may be inferred, that the final contraction of muscles is not the coagulation of the blood contained in them; neither is it a change in the reticular membrane, nor in the blood-vessels, because such contractions are not general throughout those substances. The coagulation of the blood is a certain criterion of death. The reiterated visitations of blood are not essential to muscular irritability, because the limbs of animals, separated from the body, continue for a long time afterwards capable of contractions, and relaxations.

The chemical combinations of living matter are transient, and not renewable.

The constituent elementary materials of which the peculiar animal and vegetable substances consist, are not separable by any chemical processes hitherto instituted, in such manner as to allow of a recombination into their former state. The composition of these substances appears to be naturally of transient duration, and the attractions of the elementary materials which form

form the gross substances, are so loose and unsettled, that they are all decomposed without the intervention of any agent, merely by the operation of their own elementary parts on each other.

An extensive discussion of the chemical properties attaching to the matter of muscle would be a labour unsuited to this occasion; I should not, however, discharge my present duty, If I omitted to say, that all such investigations can only be profitable when effected by simple processes, and when made upon the raw materials of the animal fabric, such, perhaps, as the albumen of eggs, and the blood. But, until by synthetical experiments the peculiar substances of animals are composed from what are considered to be elementary materials, or the changes of organic secretion imitated by art, it cannot be hoped that any determinate knowledge should be established upon which the physiology of muscles may be explained. Such researches and investigations promise, however, the most probable ultimate success, since the phenomena are nearest allied to those of chemistry, and since all other hypotheses have, in their turns, proved unsatisfactory.

Difficulties attending chemical research into these objects.

*Facts and Experiments tending to support and illustrate the preceding Argument.*

An emaciated horse was killed by dividing the medulla spinalis, and the large blood-vessels under the first bone of the sternum.

Temperature of the primary fluids in different animals. The horse.

The temperature of the flowing blood was  $103^{\circ}$

Spleen - - - 103

Stomach - - - 101

Colon - - - 98

Bladder of urine 97

Atmosphere - 30.

Three pigs, killed by a blow on the head, and by the immediate division of the large arteries and veins, entering the middle of the basis of the heart, had the blood flowing from these vessels of  $106$ ,  $106\frac{1}{2}$ , and  $107^{\circ}$ ; the atmospheric temperature being at  $31^{\circ}$ .

Pigs.

An ox, killed in a similar manner, the blood  $103$ ; atmosphere  $50^{\circ}$ .

An ox.

Three sheep, killed by dividing the carotid arteries, and internal jugular veins: their blood  $105$ ,  $105$ ,  $105\frac{1}{2}^{\circ}$ ; atmosphere  $41^{\circ}$ .

Sheep.



Frogs.

Three frogs, kept for many days in an equable atmosphere at  $54^{\circ}$ ; their stomachs  $62^{\circ}$ .

Fluid of dropsy.

The watery fluid issuing from a person tapped for dropsy of the belly  $101^{\circ}$ : the atmosphere being  $43^{\circ}$ , and the temperature of the superficies of the body at  $96^{\circ}$ .

These temperatures are considerably higher than the common estimation.

Experiment to shew that the volumes of muscles are increased during action.

A man's arm being introduced within a glass cylinder, it was duly closed at the end which embraced the head of the humerus; the vessel being inverted, water at  $97^{\circ}$  was poured in, so as to fill it. A ground brass plate closed the lower aperture, and a barometer tube communicated with the water at the bottom of the cylinder. This apparatus including the arm, was again inverted, so that the barometer tube became a gage, and no air was suffered to remain in the apparatus. On the slightest action with the muscles of the hand, or fore-arm, the water ascended rapidly in the gage, making librations of six and eight inches length in the barometer tube, on each contraction and relaxation of the muscles.

Crimping of fish cannot be effected after rigidity of death.

The remarkable effects of crimping fish by immersion in water, after the usual signs of life have disappeared, are worthy attention; and whenever the rigid contractions of death have not taken place, this process may be practised with success. The sea fish destined for crimping are usually struck on the head when caught, which, it is said, protracts the term of this capability; and the muscles which retain this property longest are those about the head. Many transverse sections of the muscles being made, and the fish immersed in cold water, the contractions called crimping take place in about five minutes; but, if the mass be large, it often requires thirty minutes to complete the process.

Experiment.

Two fish were scored and one of them crimped. Its specific gravity, and also its absolute weight, was increased by crimping.

Two flounders, each weighing 1926 grains, the one being in a state for crimping, the other dead and rigid, were put into water at  $48^{\circ}$ ; each being equally scored with a knife. After half an hour, the crimped fish had gained in weight 53 grains, but the dead fish had lost 7 grains. The specific gravity of the crimped fish was greater than that of the dead fish, but a quantity of air-bubbles adhered to the surfaces of the crimped muscles, which were rubbed off before weighing; this gas was not inflammable.

The



The specific gravity of the crimped fish 1,105  
 of the dead fish, after an equal  
 immersion in water 1,090.

So that the accession of water specifically lighter than the muscle of fish, did not diminish the specific gravity of crimped muscle, but the contrary: a proof that condensation had taken place.

A piece of cod-fish weighing twelve pounds, gained in weight, by crimping, two ounces avoirdupois; and another less vivacious piece, of fifteen pounds, gained one ounce and half\*.

Other substances crimped with like effect.

The hinder limb of a frog, having the skin stripped off, and weighing  $77\frac{1}{10}$  grains, was immersed in water at  $54^{\circ}$ , and suffered to remain nineteen hours, when it had become rigid, and weighed  $100\frac{1}{4}$  grains. The specific gravity of the contracted limb had increased, as in the crimped fish.

Six hundred and thirty grains weight of the subscapularis muscle of a calf, which had been killed two days from the 10th of January, was immersed in New River water at  $45^{\circ}$ . After ninety minutes, the muscle was contracted, and weighed in air 770 grains: it had also increased in specific gravity, but the quantity of air-bubbles formed in the interstitial spaces of the reticular membrane made it difficult to ascertain the degree.

Some of the smallest fasciculi of muscular fibres from the same veal, which had not been immersed in water, were placed on a glass plate, in the field of a powerful microscope, and a drop of water thrown over them, at the temperature of  $54^{\circ}$ , the atmosphere in the room being  $57^{\circ}$ . They instantly began to contract, and became tortuous.

Fibrils of muscles contract by application of water.

On confining the ends of another fibril with little weights of glass, it contracted two-thirds of its former length, by similar treatment. The same experiment was made on the muscular fibres of lamb and beef, twelve hours after the animals had been killed, with the like results. Neither

\* I am informed that the crimping of fresh water fishes requires hard water, or such as does not suit the purposes of washing with soap. This fact is substantiated by the practice of the London fish-mongers, whose experience has taught them to employ pump water, or what is commonly called hard water.

but not by acid vinegar, nor water saturated with muriate of soda, nor strong or saline water. ardent spirit, nor olive oil, had any such effect upon the muscular fibres.

Cold renders muscles torpid. The amphibia, and coleopterous insects, become torpid at  $34^{\circ}$ . At  $36^{\circ}$  they move slowly, and with difficulty; and, at a lower temperature their muscles cease to be irritable. The muscles of warm-blooded animals are similarly affected by cold.

Muscles of frogs irritable after freezing. The hinder limbs of a frog were skinned and exposed to cold at  $30^{\circ}$ , and the muscles were kept frozen for eight hours, but on thawing them, they were perfectly irritable.

The same process was employed in the temperature of  $20^{\circ}$ . and the muscles kept frozen for twelve hours, but that did not destroy the irritability.

Heat deprives muscles of their irritability; In the heat of  $100^{\circ}$ , the muscles of cold-blooded animals fall into the contractions of death; and at  $110^{\circ}$ , all those of warm blood, as far as these experiments have been extended. The muscles of warm-blooded animals, which always contain more red particles in their substance than those of cold blood, are soon deprived of their irritability, even although their relative temperatures are preserved; and respiration in the former tribe is more essential to life than in the latter.

as do poisons, Many substances accelerate the cessation of irritability in muscles when applied to their naked fibrils, such as all the narcotic vegetable poisons, muriate of soda, and the bile of animals; but they do not produce any other apparent change in muscles, than that of the last contraction. Discharges of and electricity. electricity passed through muscles, destroy their irritability, but leave them apparently inflated with small bubbles of gas; perhaps some combination obtains which decomposes the water.

Experiments on muscles immersed in fluids. The four separated limbs of a recent frog were skinned, and immersed in different fluids; viz. No. 1, In a phial containing six ounces by measure of a saturated aqueous solution of liver of sulphur made with potash; No. 2, In a diluted acetic acid, consisting of one drachm of concentrated acid to six of water; No 3, in a diluted alkali, composed of caustic vegetable alkali one drachm, of water six ounces; No. 4, in pure distilled water.

The

The phials were all corked, and the temperature of their contents was  $46^{\circ}$ .

The limb contained in the phial No. 1, after remaining twenty minutes, had acquired a pale red colour, and the muscles were highly irritable.

The limb in No. 2, after the same duration, had become rigid, white, and swollen; it was not at all irritable. By removing the limb into a diluted solution of vegetable alkali, the muscles were relaxed, but no signs of irritability returned.

No. 3, under all the former circumstances, retained its previous appearances, and was irritable, but less so than No. 1.

No. 4 had become rigid, and the final contraction had taken place.

Other causes of the loss of muscular irritability occur in pathological testimonies, some examples of which may not be ineligible for the present subject. Workmen whose hands are unavoidably exposed to the contact of white lead, are liable to what is called a palsy in the hands and wrists, from a torpidity of the muscles of the fore arm. This affection seems to be decidedly local, because, in many instances, neither the brain, nor the other members, partake of the disorder; and it oftenest affects the right hand. An ingenious practical chemist in London has frequently experienced spasms and rigidity in the muscles of his fore arms, from affusions of nitric acid over the cuticle of the hand and arm. The use of mercury occasionally brings on a similar rigidity in the masseter muscles.

Muscular irritability destroyed by other causes.

A smaller quantity of blood flows through a muscle during the state of contraction, than during the quiescent state, as is evinced by the pale colour of red muscles when contracted. The retardation of the flow of blood from the veins of the fore arm, during venæsection, when the muscles of the limb are kept rigid, and the increased flow after alternate relaxations, induces the probability, that a temporary retardation of the blood in the muscular fibrils takes place during each contraction, and that its free course obtains again during the relaxation. This state of the vascular system in a contracted muscle, does not, however, explain the diminution of its bulk, although it may have some influence on the limb of a living animal.

Less blood flows through a contracted muscle.

When



A contracted muscle is less sensible.

When muscles are vigorously contracted, their sensibility to pain is nearly destroyed; this means is employed by jugglers for the purpose of suffering pins to be thrust into the calf of the leg, and other muscular parts with impunity: it is indeed reasonable to expect, *a priori*, that the sensation, and the voluntary influence, cannot pass along the nerves at the same time \*.

Moral causes influence the muscles in the human species.

In addition to the influences already enumerated, the human muscles are susceptible of changes from extraordinary occurrences of sensible impressions. Long continued attention to interesting visible objects, or to audible sensations, are known to exhaust the muscular strength: intense thought and anxiety, weaken the muscular powers, and the passions of grief and fear produce the same effect suddenly: whilst the contrary feelings, such as the prospect of immediate enjoyment, or moderate hilarity, give more than ordinary vigour.

Mental as well as muscular actions may by habit become automatic,

It is a very remarkable fact in the history of animal nature, that the mental operations may become almost automatic; and, under such habit, be kept in action, without any interval of rest, far beyond the time which the ordinary state of health permits, as in the examples of certain maniacs, who are enabled without any inconvenience, to exert both mind and body for many days incessantly. The habits of particular modes of labour and exercise are soon acquired, after which; the actions become automatic, demand little attention, cease to be irksome, and are effected with little fatigue: by this happy provision of nature, the habit of industry becomes a source of pleasure, and the same appears to be extended to the docile animals which co-operate with man in his labours.

and then give little fatigue.

Voluntary and involuntary muscles.

Three classes of muscles are found in the more complicated animals. Those which are constantly governed by the will, or directing power of the mind, are called voluntary muscles. Another class, which operate without the consciousness of the mind, are denominated involuntary; and a mixed kind occur in the example of respiratory muscles, which are governed by the will to a limited extent; nevertheless the exigencies of the

\* I have often observed that a small electric shock may be received without pain through the muscles of the fore arm; but I imagined it to be owing to the want of power in such a shock to increase the contraction.—N.

animal feelings eventually urge the respiratory movements in despite of the will. These last muscles appear to have become automatic by the continuance of habit.

The uses of voluntary muscles are attained by experience, imitation, and instruction: but some of them are never called into action among Europeans, as the muscles of the external ears, and generally the occipito-frontalis. The purely involuntary muscles are each acted upon by different substances, which appear to be their peculiar stimuli; and these stimuli co-operate with the sensorial influence in producing their contractions: for example, the bile appears to be the appropriate stimulus of the muscular fibres of the alimentary canal below the stomach, because the absence of it renders those passages torpid. The digested aliment, or perhaps the gastric juice in a certain state, excites the stomach. The blood stimulates the heart, light the iris of the eye, and mechanical pressure seems to excite the muscles of the œsophagus. The last cause may perhaps be illustrated by the instances of compression upon the voluntary muscles, when partially contracted, of which there are many familiar examples. Probably the muscles of the ossicula auditûs are awakened by the tremors of sound; and this may be the occasion of the peculiar arrangement observable in the chorda tympani, which serves those muscles.

These extraneous stimuli seem only to act in conjunction with the sensorial power, derived by those muscles from the gangliated nerves, because the passions of the mind alter the muscular actions of the heart, the alimentary canal, the respiratory muscles, and the iris; so that probably the respective stimuli already enumerated, only act subserviently, by awakening the attention of the sensorial power, (if that expression may be allowed,) and thereby calling forth the nervous influence, which, from the peculiar organization of the great chain of sympathetic nerves, is effected without consciousness: for, when the attention of the mind, or the more interesting passions prevail, all the involuntary muscles act irregularly, and unsteadily, or wholly cease. The movements of the iris of the common parrot is a striking example of the mixed influence.

Voluntary actions require education; the involuntary are caused by stimuli.

Stimuli seem to excite sensorial power in some respect resembling the passions.

The muscles of the lower tribes of animals, which are often entirely supplied by nerves coming from ganglions, appear of this class; and thus the animal motions are principally regulated

Lower tribes of animals act by external excitements.

lated

lated by the external stimuli, of which the occurrence seems to agree with the animal necessities: but the extensive illustrations which comparative anatomy affords on this point, are much too copious for any detail in this place.

The nervous power seems constantly active through life.

There are two states of muscles, one active, which is that of contraction, the other, a state of ordinary tone, or relaxation, which may be considered passive, as far as it relates to the mind; but the sensorial or nervous power seems never to be quiescent, as it respects either the voluntary or involuntary muscles during life. The yielding of the sphincters appears to depend on their being overpowered by antagonist muscles, rather than on voluntary relaxation, as is commonly supposed.

I have now finished this endeavour to exhibit the more recent historical facts connected with muscular motion.

Conclusion.

It will be obvious to every one, that much remains to be done, before any adequate theory can be proposed. I have borrowed from the labours of others, without acknowledgement, because it would be tedious to trace every fact, and every opinion to its proper authority: many of the views are perhaps peculiar to myself, and I have adduced many general assumptions and conclusions, without offering the particular evidence for their confirmation, from a desire to keep in view the remembrance of retrospective accounts, and to combine them with intimations for future research. The due cultivation of this interesting pursuit cannot fail to elucidate many of the phenomena in question, to remove premature and ill-founded physiological opinions, and eventually to aid in rendering the medical art more beneficial, by establishing its doctrines on more extensive and accurate views of the animal economy.

## VI.

*On the Measure of Mechanic Power. In a Letter from  
Mr. J. C. HORNBLLOWER.*

To Mr. NICHOLSON.

DEAR SIR,

Measure of  
mechanic power  
or effect.

I AM glad to find that somebody has seconded my motion concerning *horse power*, and I hope the subject will not be dismissed



missed until it has passed the unanimous assent of both theoretic Measure of mechanic power or effect. and practical mechanics; and I must here express my acknowledgments to Mr. Gregory for his improved method of deciding the question. It is of absolute importance, that the draught be in a circular direction, and also that the radius of the circle be given; for no position was ever more demonstrable than that the less the radius the less can a horse (whose sides are equal) exert his faculty of traction.

But I must beg leave to use a little freedom (in no wise unbecoming, I hope,) in adverting to what Mr. Gregory in conjunction with Professor Robison has advanced on the subject of Mr. Smeaton's mode of defining *mechanic powers* and *mechanical effect*; and I am surprised that among men of talent and assiduity there can be a difference of opinion. It seems to me, that if any reason can be found, it must be that we do not understand the subject, and, perhaps I may give a decided proof of it in what I shall advance concerning it.

However, I am sure that when a ball of cast iron, of twenty inches diameter is elevated by means of a pinion and wheel connected to the sides of a pair of sheers, and left to hang there a little while,—if I cut the rope that sustains it, it will fall freely a certain height, in a certain portion of time, and would dash a faulty cylinder of a steam engine in pieces. And I am so well satisfied that there was a certain tendency in this ball to fall towards the centre of the earth, that I need not take a moment to examine the truth of it; but that the destruction of the cylinder was occasioned by the ball falling from the point to which it was raised, (how it was raised is no part of the subject.) Then I say, that if the ball had not fallen from the height it was, or if the ball had not been so heavy as it was, or if it had had its velocity retarded by any means, the cylinder would not have been broken. I will add, that had this ball been a true sphere, and the cylinder had not been there, but a certain curved surface in its place, which should have received the ball to prevent it impinging in the line of descent, it would have been turned out of its course conformably to the nature and position of the curve; and all that is above common apprehension in this matter, is, that while the ball was falling, its velocity was increasing every instant, and that when it met with the curve (if it gave it an horizontal course) it would proceed with uniform velocity

just

Measure of  
mechanic power  
or effect.

just twice the space through which it fell, allowing the same time for this horizontal course as it had in falling its perpendicular height, and that its tendency is to continue that course for ever.

From this statement of the law of bodies in motion, (so far as it goes) I think we need not be very diffident in saying, that it is from similar facts, (though less philosophically observed) that we obtain our primary ideas of motion. The apple falling from the tree is a very good instance to the point, and it would not require a very extraordinary stretch of genius to apply such an accident as that to any thing like the pile engine, stamping press, &c.

But there are certain speculative mechanics, who in their mode of accounting for effects like what are here stated, have chosen to adopt terms of a very different import, and for some latent reason wish to keep gravitation out of sight. The writer of the article Dynamics in the Supplement of the Encyclopedia Britannica, calls it pressure, and by his way of philosophising in the explanation of the measure of mechanic power, has (in my opinion) laboured to make it as mysterious as possible.

I must for the sake of those of your readers who have not the work to refer to, quote a few passages now and then from the above popular work, and here I would refer them to the article on Machinery, where he begins by stating, that different notions have been entertained on this subject by Leibnitz, des Cartes, and other eminent mechanics of the last century; and adds, "*that some of the most eminent practitioners of the present times (for we must include Mr. Smeaton in the number) have given measures of mechanical power in machinery, which we think inaccurate, and tending to erroneous conclusions and maxims.*"

He then proceeds to explain and demonstrate the true measure of mechanic power, and he begins by supposing a man pressing uniformly on a mass of matter for a certain time, and going on with the subject takes occasion to distinguish between *the weight of a body and its heaviness*; and, towards the latter end of that section he comes to some sort of a conclusion of the subject, so far as to say what is the real measure of mechanic power: I see I must make endless quotations if I regard the very letter of his argument, but I hope I shall be excused; however I will quote the last paragraph verbatim.

But

Relating



Relating to the pressure of the man just mentioned, he says, *Measure of*  
 “but farther we know, that this pressure is the exertion \*, *mechanic power*  
 we have no other notion of our own force, and our notion of *or effect.*  
 gravity, of elasticity; or any other natural force is the same.  
 We also know, that the continuance of this exertion fatigues  
 and exhausts our strength as completely as the most violent  
 motion. A dead pull as it is called of a horse at a post fixed  
 in the ground, is a usual trial of his strength. No man can  
 hold out his arm horizontally for much more than a quarter of  
 an hour, and the exertion of the last minutes gives the most  
 distressing fatigue, and disables the shoulder for action for a  
 considerable time after. *This is therefore an expenditure of*  
*mechanical power in the strict primitive sense of the word.* Of  
 this expenditure we have an exact and adequate effect and  
 measure in the quantity of motion produced, that is in the  
 product of the quantity of matter by the velocity generated  
 in it by this exertion. And it must be particularly noticed,  
 that the measure is applicable even to cases where no motion  
 is produced by the exertion; that is, if we know that the  
 exertion which is just unable to start a block of stone lying on  
 a smooth pavement, but would start it if increased by the  
 smallest addition, and if we know that this would generate  
 in a second 32 feet of velocity in 100 pounds of matter, we  
 are certain that it was a pressure equal to the weight of this  
 100 pounds. It is a good measure, though not immediate,  
 and may be used without danger of mistake when we have no  
 other.”

I should not have quoted so much of this section, if it had  
 not been that I think it contains an unequivocal interpretation  
 of the writers notion of the true measure of mechanic power, and  
 at once exhibiting, in my proud opinion, the fallacy of the  
 doctrine in *toto*. What! shall mere muscular exertion,  
 whether of horse or man, be esteemed even an auxiliary to  
 get the conception of the nature of the thing; how much less  
 then shall it be set forth as the thing itself? In perfect con-  
 sonance with this writer, Mr. Gregory, page 152, Philosophical  
 Journal, Vol. XI. says, suppose that a horse while standing  
 still, sustains by means of a rope and simple fixed pulley, a  
 mass of an hundred weight, and thus keeps it suspended at  
 the top of a well for the space of a minute; neither the  
 animal nor the weight moves; but shall we say, in conformity

\* Article Machinery, Supplement, 4th sec.



Measure of  
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as it would seem with Mr. Smeaton's measure, that there is no power expended, no effect produced. On the contrary we know there is a power expended, and that effect, if sufficiently long continued, would completely tire the horse. Then let us have a *post* instead of the horse, and surely that will not tire, and what will be the consequence then? why then there will be no power expended, and no effect produced; and I beg leave to ask my opponents, what is the power expended when a horse or other animal is placed there to sustain the weight? is it any more than the expence of nervous or muscular action; and has that any analogy with a weight descending through a given space, either uniformly or accelerated? and, I ask again, what is the effect produced more than what is produced by the post? the horse does but keep the weight from dropping into the well, and the post will do the same; indeed you may say, that when you hang up your hat, that the pin which sustains it, prevents it from falling, as does the horse the mass in the well, and that therefore there must be some power expended on the pin.

It is really difficult to be grave on this occasion; but I feel myself restrained by the magnitude of the subject, and its importance to the community. Professor Robison says, when a man holds out his arm horizontally, the exertion towards the end of a quarter of an hour gives the most distressing fatigue, and then says this is an expenditure of mechanical power, which I shall take the liberty to deny for the present. But is it such a mechanical power as Smeaton's, or in fine, is it a power made up of a mass of matter moving with any determinate velocity either uniform or accelerated? If the learned Professor intended to familiarize the doctrine to people of common sense, he could not have chosen a more indirect and perplexing example.

But let us attend a little further to the subject in the fourth section of the article Machinery, Sup. Ency. Brit. There he says, that "when a man supports a weight for a single instant, he certainly balances the pressure or action of gravity on that body," by the way here is a great want of precision in the expression, "*pressure or action of gravity*," as if they were synonymous terms, whereas *pressure* certainly denotes *repulsion*, if the term will bear any definition at all, and to explain the term *gravity* if it will not admit of *attraction*, I am

sure

sure it cannot be called repulsion, but to proceed, "and he continues this action as long as he continues to support it, and we know, that if this body were at the end of a horizontal arm turning round a vertical axis, *the same effort* which the man exerted in merely carrying the weight, if now exerted on the body by pushing it horizontally round the axis, will generate in it the same velocity which gravity would generate by its falling freely." Measure of  
mechanic power  
or effect.

A more erroneous proposition was never introduced to the theory or practice of mechanics. What, is there no difference in a man carrying a load on his shoulder, and putting it into a truck? or to come nearer the Professor's proposition, let the man who has to carry two hundred pounds for one mile be permitted to take the weight from his shoulders, and rest it on the arm of any thing like a horse wheel, perfectly detached from the mill gear, let the gudgeons be oiled, and then let him "*push*" it horizontally round its axis until he has travelled a mile.

Now without asking the man which he likes best, let us see what he does by placing his load on the arm of the horse-wheel, and pushing it round. Why, he certainly overcomes the additional friction which his load has added to the weight of the wheel, and that is all, and if you will let us have gudgeons which have no friction, the man need not to walk far to push the horizontal arm into perpetual motion.

But now for the monstrous conclusion by this proposition. "If the man's exertion was employed to generate motion instead of counteracting gravity, he would generate during that minute the same motion that gravity would; that is  $60 \times 32$  feet velocity per second in a mass of 30 pounds. There would be 30 pounds of matter moving with the velocity of 1920 feet per second. We would express this production or effect by  $30 \times 1920$ , or by 57600 as the measure of the man's exertion during the minute."

Here is evidently a typographical error, *second for minute*, but when we admit suppositions for the sake of illustration, there ought to be some conformity in the supposition to the fact it is intended to illustrate: then I would ask, where's the man who can generate 32 feet of velocity in a mass of 30 pounds in one second? to be sure he can let it fall a second, but he cannot carry it 32 feet in a second; but he says, "we

Measure of M would express the production or effect by  $30 \times 1920$  or  
 mechanic power 5760 as the measure of the man's exertion during the minute."  
 or effect.

Sir, it is more than even one of Boulton and Watts' horses can do.—Well may he say, "such an exertion will completely exhaust a man's strength."

He then goes on to consider "*more narrowly* what a man *really* does, when he performs what Mr. Smeaton allows to be the production of a measurable mechanical effect. Suppose a weight of 30 pounds hanging by a cord which passes over a pulley, and that a man taking this cord over his shoulder, turns his back to the pulley, and walks away from it, we know that a man of ordinary force will walk along raising this weight at the rate of about sixty yards in a minute, or a yard in every second, and that he can continue to do this for eight or ten hours from day to day, and that this is all he can do without fatigue. Here are 30 pounds raised uniformly 180 feet in a minute, and Mr. Smeaton would express this by  $30 \times 180$  or 5400, and would call this the measure of the mechanical effect, and also of the expenditure of power. This is very different from our measure 57600."—Yes, but I hope not the less conclusive on that account.

It is wholly incomprehensible to me why those men (who have certainly a right to controvert any proposition which appears to them erroneous) should take up the subject, assuming points which the doctrine advanced by Mr. Smeaton has nothing to do with. It is clear that all this animal exertion comes at last to the law of bodies falling in accelerated velocity, which Smeaton allows to be a distinct consideration, as he says, "if the weight descends quickly, it is sensibly compounded with another law, viz. the law of acceleration by gravity." But how inconsistent is it to go about to elucidate the laws of bodies in motion by the action of a man or a horse. What is the expenditure of animal power but a waste of what has been usually termed by anatomists nervous spirits, or perhaps an inceptive disorganization of the constituent parts of the ligaments and muscles? in short, we may compare it to contractions, inflammation, and gangrene, but never to the momentum acquired by a body moving through a certain space in a certain time. It is remarkable, that all this reasoning is about that of which Mr. Smeaton has never said a word, except in his illustration of the mechanic power  
 necessary



necessary to give velocity to heavy bodies, where he supposes a man pushing an iron ball on an extended plane, and this not to try his muscular force, or to see what he can do without tiring, but merely in elucidation of the doctrine he sets out with.

Measure of  
mechanic power ]  
or effect.

I have not time at present to pursue the subject to the extent I wish, and to enter on the ground of the mistake of this great man: nor let it be imagined for a moment that I have availed myself of his everlasting absence, to call in question what he has advanced in refutation of a supposed error. It would have remained as it was unto a distant period, had it not been, that I see Mr. Gregory advancing the same opinion, which no doubt he will defend or desert.

I am, Dear Sir,

Your obedient Servant,

J. C. HORNBLOWER.

If Mr. Gregory should read this, I shall be glad if he will set me right as to the identity of *animal exertion* and *mechanic power*.

## VII.

*An Investigation of all the Changes of the variable Star in Sobieski's Shield, from five Years Observations; exhibiting its proportional illuminated Parts, and its Irregularities of Rotation; with Conjectures respecting unenlightened heavenly Bodies. By EDWARD PIGOTT, Esq. Abridged from the Philosophical Transactions for 1805.*

THE author begins his memoir with an investigation of the periods of change in the variable star in Sobieski's shield, of which the right ascension was  $279^{\circ} 9\frac{1}{2}'$ , and its declination south  $5^{\circ} 56'$ , for end of June 1796. Its rotation on its axis in 1796 was estimated at  $62\frac{3}{4}$  days, from a mean of six observations of its greatest and least brightness. In the present paper he gives about 26 similar determinations, most of them the results of very accurate observations made in the year 1796, 1797, 1798, 1799, and 1801. From all these results

The variable  
star in Sobieski's  
shield, revolves  
on its axis in  
 $61\frac{1}{2}$  days.

it

it was found that the disagreements between the periods of change, as deduced from its full brightness, were much greater than those deduced from its least illumination. The former gave the mean period 63 days, and the latter  $59\frac{3}{4}$ ; and the mean of these, namely  $61\frac{1}{2}$  days, agreeing with the former determination to  $\frac{1}{4}$  day, is as near as could be expected in observations of this nature.

The places of full and least brightness do not equally divide the star's circumference; they divide it as 7 to 8.

The author in the next place proceeds to examine some other of the changes to which this star is subject. By tabulated observations through the same series of years, he finds that the time of decrease, from the middle of its full brightness to the middle of its least, is on a mean of 34 days; and that the time of its increase, from the middle of its least brightness to the middle of its full, is in like manner only 27 days. The sum of these numbers amounting to the period 61, shews their probable exactness. These compared and combined with the former determinations of 1796, give a mean of the whole  $33\frac{1}{2}$  and  $29 -$  days. As it thus appears that the time of the decrease is longer than that of the increase, it follows of course, that the places of the full and least brightness are not situated at the distance of half the circumference from each other; and the like circumstance Mr. P. affirms is found to be the case with most, if not all the variable stars.

Other variable stars are similarly affected. The luminous parts are themselves variable.

The next particulars to be reviewed were the durations of its brightness without any perceptible change, while at its maximum and minimum. These determinations required a tolerable succession of observations; where that is not the case he has omitted them in his tables. From these it is found in general, that when the degree of brightness at its maximum is less than usual, and its minimum not much decreased, the changes take place but very slowly, and cannot be settled with much accuracy unless the observations have been made frequently and with great attention. He accordingly passes again over the series of years, shewing the dates of its magnitudes when at its full brightness, and also when at its least brightness, and he tabulates at the end of the first part of his paper, all the different changes that have been examined. The words in the first column or compartment describe them; in the second column the present results are exhibited; in the third are the results of the former observations; and in the last column is placed a mean of both computed proportionally, according to the number of observations of each.

TABLE VIII.

	Days.	Days.	Days on a mean.	Table of the general affections of the variable star.
Rotation on its axis	61½	62½	62—	
Duration of brightness, at its maximum, without any perceptible change	8+	14	9½	
Ditto, when it does not attain its usual brightness	20—	—	—	
Duration of brightness at its minimum, without any perceptible change	9—	9	9	
Ditto, when it does not decrease so much as usual	20—	—	—	
Decrease in time, from the middle of its full brightness to the middle of its least	34	28	33+	
Increase in time, from the middle of its least brightness to the middle of its full	27+	35	29—	
Extremes of its different degrees of brightness; with a mean of its usual variations	5+ 9 or 0	5+ 7.8	5. 6	

The author having thus settled with considerable precision these essential variations of the star, proceeds to examine some of its other phenomena, particularly one which is common to most of the changeable stars, and likewise in some degree to our sun, namely, that the times of their periodical returns of brightness are in general irregular,—a circumstance so interesting as to engage our attention, and which induced Mr. P. to make the succession of observations, in the hope of discovering its nature and cause. With this view he has proceeded to tabulate the series of years, so as to ascertain the apparent rotations in days from the observed middle times of its full brightness, and also from the observed middle times of its least brightness, for single periods. From these it appears, that the periodical returns of brightness are uncommonly fluctuating, and that the differences between the extremes are very considerable; to account for which he offers the follow-

The changes in the luminous parts occasion the periods singly taken to differ from each other.



ing explanations, suggesting previously a few plausible conjectures, and some inferences arising from the observations themselves\*.

Assumed positions:

1. The stars are opaque. 2. They have regular rotations. 3. Their luminous appearance is caused by an atmosphere like that of the sun. 4. The luminous parts are sparingly dispersed in the star here treated of:

1st. That the body of the stars are dark and solid.

2d. Their real rotations on their axes are regular.

3d. That the surrounding medium is by times generating and absorbing its luminous particles in a manner nearly similar to what has been lately so ingeniously illustrated by the great investigator of the heavens, Dr. Herschel, with regard to the sun's atmosphere.

4th. That these luminous particles are but *sparingly dispersed* in the atmosphere surrounding the variable star of Sobieski, appears from the star being occasionally diminished to the 6.7 magnitude, and much less. July 4, 1799, it was of the 7th; September 15, 1798, and August 9, 1803, *of the 9th, if not invisible*. (See Table VII †.) Does not this indicate a very small portion of light on its *darkened hemisphere*?

5. Probably small patches:

5th. And may we not with much plausibility consider them as spots, somewhat circular, or of no great extent? for even on its *brightest hemisphere* the *duration* of its full lustre is, on a mean, only  $9\frac{1}{2}$  days of the 62, or about one-sixth and  $\frac{1}{2}$  of its circumference. (See Table VIII. page 140.) The dimensions therefore of the parts enlightened seem much circumscribed, and can be tolerably estimated, and consequently may be represented very small, particularly, if the *powerful effect of a little light* and the *length of time* a bright spot is remaining in view be taken into consideration.

6. Changeable in their nature:

6th. And a further ground of presumption that those principal bright parts are but slight patches is, that they undergo *perpetual changes*, and also that such changes are very visible to us, for most probably they would be imperceptible, were not the bright parts contrasted by considerable intervals or diminutions of light.

7. And deducible from the phenomena.

7th, and last. We may obtain some idea of the *relative situation or intervals between* these bright parts, by the observa-

\* The rest of the paper is given without abridgement, and the author himself speaks in the first person.

† The author refers to his tables in the Transactions, of which the abridged result has been here given.

tions

tions of the increase and decrease of brightness, as thereby the changes and times elapsed are pointed out. (See Table V. page 136; and Phil. Transf. for 1797.)

I have tried practically the effect of the above suppositions, by placing small white spots on a dark sphere, which being revolved round represented the various changes as nearly as could be expected: proceeding therefore with these and other considerations, I shall make ideal drawings of the star with the small illuminated parts in its atmosphere, and apply to them some of the actual observations from both the preceding tables, having always in view that each period may, more or less, require a different disposition of spots, in consequence of their constant changeability.

### 1st View.

Plate XIII. Fig. 1, A B, the star's polar axis, round which its rotation takes place in 62 days from C to D.

C D, its equator, the 360 degrees of which being revolved in 62 days, gives nearly  $5\frac{3}{4}$  degrees for each day's motion; the brightest part or spot is represented as centrally facing us, and accordingly shewing the star in its greatest lustre. Were this bright spot and the other parts to remain *unchangeable*, they would after having completed the revolution of 360 degrees, or 62 days, (the star's rotation on its axis,) appear again as at first, and at every return continue to give exact periodical times, as was nearly the case in 1799, between August and October, (see Table IX. p. 142); but if the spot becomes obscure, and another brightens up in a different place, this latter will make the star appear at its next full splendour either sooner or later than the real rotation according to its position, thus,

### 2d View.

Fig. 1. A full brightness having been shewn by the same spot, it afterwards loses its light, and another as bright is produced 5 days motion (or 29 degrees) preceding it at E, see Fig. 2. This latter, when turned centrally to the earth, will appear 5 days sooner than the former one, now obscured, (here marked P,) and show the star at its full lustre, making the rotation 57 days instead of 62, which was the case in 1796, the observed revolution between September 17 and November 13. (See Table IX.)

Experiment with a sphere.

View of the star at its greatest brightness.

Anomaly from another bright spot causing an apparently short period.



## 3d View.

Another causing  
a period appar-  
ently long.

Fig. 3. We will now apply a case of an interval of too great length, that of 72 days: the spot *m* alone having shewn us the star in its full lustre, its light disappears during the revolution, and another brightens forth ten days (or 58 degrees) following it at *H*; when *m* returns to face us again in 62 days it being obliterated, the star will appear obscured, and not recover its splendour until the new brightened part *H* becomes central, which being *ten days later* than the position in which *m* was seen, makes the revolution 72 days instead of 62, as was observed between July 14 and September 24, 1801. (See Table IX.) In the above case the alterations took place while behind the star, otherwise some irregularities would have been perceived, as will later be noticed. The same reasoning with proper alterations will, I apprehend, account for the other revolutions, yet I shall soon again resume the subject with regard to a *series* of the greatest irregularities; at present let us proceed to take a few views of the intervals of its *least brightness*, which, contrary to my expectation, I find much more difficult to explain than those of the full, although the results disagree less among themselves. The darkened face of the star is here represented with a few small changeable bright spots, placed in general at a proper distance, so as to keep up an uninterrupted increase and decrease of light with regard to us, and are also made to correspond with several other observations.

## 4th View.

Greatest period  
explained; be-  
tween intervals  
of least bright-  
ness.

Fig. 4 is to explain the greatest interval of 74 days, between July 4th, and September 16th 1799. (See Table XI.) The darkened hemisphere here exhibited in its *minimum* July 4th, with the following spots, *w* nearly gone off, next a small one *l*, then another *P* of a similar size, preceding the centre a day or two, (or a few degrees; and lastly a bright one at *D*, just appearing. During the rotation, *D* losing its light and the *P* becoming *much brighter*, the star at its next return in 62 days, when at its first position, must of course appear much brighter, (See fig. 5) but by the retiring of *l* and *P* continues to diminish in lustre till the appearance of some large spot from the other hemisphere; which taking place 12 days afterwards, will, (when this time is added to the 62 already revolved) make the revolution



revolution of 74 days, as required; for a view of a short interval, for the present let that of 56 days be taken between August 21st and October 16th 1801. (See Table X.)

5th. View.

The least brightness or *minimum* is represented by fig. 6, Least brightness, when the bright spots  $y$  and  $x$  at each extremity of the equatorial diameter are mutually but just in sight and a minute one,  $r$  alone on its surface preceding  $y$  by 6 days motion:  $nn$ , are other middling sized spots near  $x$ , but preceding it; they cannot for the present be seen, being on the opposite or bright hemisphere. The spot  $x$  during the stars revolution having lost its light, and  $r$  being considerably increased, the next *minimum* will be between  $nn$  and  $r$ , (instead of  $x$  and  $y$ .) See fig. 7; and by the retiring of  $nn$  the *diminution* of the star's light will continue to take place only until the re-appearance of  $r$ , at the place where  $y$  was, which being 6 days sooner than the former position, (See fig. 6,) reduces the rotation to 56 days. All the foregoing views are from unconnected periods, where only the ultimate returns of each appearance have been attended to; but now, I shall examine a long interval with many intermediate changes, that between June 18th, and September 17th 1796, wherein are included the most intricate irregularities and vicissitudes: these observations are already pointed at full length in the Philosophical Transactions for 1797, and therefore can at any time be inspected: indeed, I then little thought they would ever become of further use, but that of stating facts, to which, however, I have always been very partial, and particularly so, after having experienced the advantage of Maraldi's printed observations on the variable star in Hydra, as it was partly by them that I ascertained the periodical returns of brightness of that star, and which flattered me the more, as Maraldi himself had been less successful in the attempt; See Phil. Trans. for 1786. Yet in the present Paper I have omitted all such details, being aware they might be thought too voluminous, but hope at some future time the Society will honour them with a place in their library.

The first sketch, Plate XIV. represents, for June 13, 1796, the comparative size of the bright spots supposed to surround the star, but here extended at full length; the next eight following are

Explanation by the figures of the periodical variations and irregularities of light in the star in Sobieski's shield.

are spherical views, on an enlarged scale, for each quarterly, rotation or less, shewing the principal changes, as expressed in the adjoining remarks, and corresponding with the observations; these being taken from my printed paper, as already mentioned, are marked in italics. It will be seen that the spots by which the changes are principally regulated, are placed at equal distances, yet intermediate ones might also frequently be inserted without occasioning any objection, but that of rendering the explanations more complex.

### REMARKS ON PLATE III.

Fig. 2. "*June 18th. Full brightness Mag. bright 5th,*" before or after which date the star would appear less bright, by the spot E being removed from the centre, and one of the others out of view.

Fig. 3. "*July 3d, 15 days or  $\frac{1}{4}$  rotation being elapsed since June 18th, 5th Mag, a little decreased*" by the removal of the brightest spot E, the *h* being much less.

Fig. 4. "*July 19th, 16 days or  $\frac{1}{4}$  rotation 5.6 Mag. still decreased,*" N being much less than *h*, now gone off. *A slight minimum.*"

Fig. 5. "*July 27th, 8 days of the rotation, 5 Mag. rather increased*" by the considerable increase of N since four days, with the addition of F, *a slight full brightness.*

Fig. 6. "*Aug. 3d, 7 days of rotation, 5.6 Mag. decreased* by the going off of N, the E, which is now reappearing, being reduced to much less than F.

Fig. 7. "*Aug. 19th, 16 days or  $\frac{1}{4}$  rotation, 5.6 Mag. again decreased,*" by the removal of F, by E being much less, and by the *h* also being considerably diminished.

Fig. 8. "*Sept 3d, 15 days or  $\frac{1}{4}$  rotation, 6 Mag. still more decreased,*" by the *h* being much less than E, which is now going off, and N scarcely reappearing, *another minimum.*

Fig. 9. "*Sept. 17th, 14 days or near  $\frac{1}{4}$  rotation, 5 Mag. full brightness considerably increased,*" by N having retained its increased brightness of July 27, and now facing us centrally.

1st, Thus are exhibited, the two short intervals of its full brightness, one between June 18 and July 27, of 39 days, and the other between July 27 and Sept. 17, of 52 days. See Table IX.



2dly, The interval of 46 days between the two *minima* of July 19 and Sept. 3; See Table X.

3dly, The long decrease of 38 days between July 27 and Sept. 3, and

4thly, The rapid increases of 3 and 14 days between the 19 and 27th of July, and the 3d and 17th of September,

As also the other intermediate changes, yet I must again repeat, particularly as a few days error may occasionally proceed from the observations, that by these sketches it is not meant to give exact drawings of the size, distances or alterations of the spots, but merely to shew how the changes may take place, as, I believe, nothing of the kind has hitherto been offered to the public, either with or without corroborating observations; nor do I presume to think, that the explanations are the only ones or best can that be imagined, the more so, as they solely refer (for greater simplicity) to the star's equator, while possibly, were the spots placed in a northern or southern latitude, or permanent ones near the poles, or were a proper inclination, given to the polar axis, they might be more satisfactory: however, the materials themselves, the *observations* and *deductions* will I flatter myself ever be acceptable, and contribute to facilitate future conjectures, which from an allowable analogy may extend to similar parts of the starry system, with regard to the probability of establishing whether any of the most *irregular* or *particular* changes may not return at *six periods*, or after a certain number of rotations. I think we can entertain but slight hopes of it, owing to the *great fluctuation* of the luminous matter, as shewn by the *perpetual varying* of the *apparent* revolutions, magnitudes, &c. See Tab. IX. X. and VII. Still it is natural to suppose, that some parts of the atmosphere of this star may have a less tendency than others to become luminous, so as to promote at different times, similar appearances; and indeed that is strongly indicated by the *intervals* of the *minima* being far *more regular* than those of the *full brightness*, which, with other reasons induce us to suspect even that one of its hemispheres is less favourably constituted or qualified, than the other for the generating of these particles, although they do occasionally encroach on both sides, as appears by the observations between June and August, See Phil. Trans. for 1797, or the eight sketches of 1796, and likewise in 1797, see Tab. VII. when during *three months* it was only reduced to the

Explanation by the figures of the periodical variations and irregularities of light in the star in Sobieski's shield.



Changeable stars  
may become per-  
manent;

and others dis-  
appear.

There may be  
dark stars,

perhaps equal in  
number to the  
visible ones.

Our sun,  
though variable,  
is still very lumi-  
nous;

5 or 6 Mag. by which the degree of brightness that surrounded it, must have been nearly equal: had the causes of varying its light then ceased, it would ever have continued to appear as an unchangeable star of the 5 or 6 Mag. and such is the case of several others that *formerly have been variables*, but for many years retain a steady brightness, as  $\beta$  Geminorum,  $\delta$  Ursæ majoris,  $\alpha$  Draconis, and perhaps that in the Swan's breast, while others, after *showing their changes*, have *entirely disappeared*, owing to a total absorption of light, as the famous one in Cassiopea, in Serpentarius of 1604, that near the Swan's head, and doubtless many more. Does not this induce us to presume that there are also others, that have *never shown* a glimpse of brightness? Lastly, *new variables* may become so at different periods, by an unusual and partial increase or diminution of their bright parts, as not unlikely was the case of  $\alpha$  Ceti, Algol  $\alpha$  Herculis, &c. for these stars being by times very conspicuous, their changes, had they been always equally great, might have been easily noticed by the ancient astronomers, who observed only with the *naked eye*. A few lines above, I mentioned the probability that there existed *primary* invisible bodies or *unenlightened stars* (if I may be allowed the expression) that have ever remained in *eternal darkness*; how numerous these may be, can never be known. Would it then be too daring or visionary to suppose their numbers equal to those endowed with light? particularly when we take into contemplation the ample set of bodies visible only by reflected rays, that appertain to our own system, such as the planets, asteroides, comets, and satellites. Do not these, although but of a secondary nature, lead us to venture on the foregoing more enlarged conjecture; and moreover to suspect, that the *enlightened stars* are those that have already attained the highest degree of perfection? granting, therefore, such multitudes do really exist, clusters of them, by being collected together as in the milky-way, must intercept all more distant rays, and if free from any intervening lights, they would appear as *dark spaces* in the heavens, similar to what has been observed in the Southern Hemisphere. That so few of these obscure places are perceived, may be attributed to their being obliterated by the presence either of some scattered stars, or of other slight luminous appearances.

I have thus fully investigated the nature of this distant sun, a single one among many millions, and scarcely perceptible

to the light, yet of no less importance than our own grand luminary. But ours is still supplied abundantly with resplendent particles, while Sobieski's variable star has them most sparingly dispersed over its sphere: a scantiness that apparently must occasion to its surrounding planets, constant vicissitudes of uncertain darkness, and repletion of light and heat. How far more enviable seems our situation; I mean that which we enjoy at present; there being strong reasons to believe, that the sun's luminous appearance has been at times considerably diminished; and I have little hesitation in conceiving that it may also be reduced, at some future period to small patches, and then the apparent irregularities of its periodical rotations, which at present are only perceived by the observations of trifling dark spots, would become evidently conspicuous, particularly when seen at a distance as remote as the variable stars are from us. But such conjectural flights of fancy cannot too soon be dropt. I therefore shall conclude with observing, that these inquiries on the alterations of light of the stars have been so little discussed, that it is to be hoped they will not be discontinued; and although I have already troubled the Society with many papers concerning such changes, I nevertheless propose, ere long, having the honour of presenting them with one more, most probably my last, on this subject.

though it may  
become reduced  
in future ages.

EDW. PIGOTT.

### VIII.

*Account of a Luminous Meteor.* By a CONSTANT READER.

To Mr. NICHOLSON.

SIR,

HAVING found it frequently stated, that it might be useful, should every one who has a fair opportunity of noting with reasonable accuracy, the course and altitude of meteors, describe their appearances as well as they are able; I send you the following account to make what use of you please.

Description of  
the appearance,  
course, and du-  
ration of a lumi-  
nous meteor.

Last night (Sunday the 21st) passing along the Strand, I stopped at the door of the Crown and Anchor, the vacant space before it lately caused by the pulling down of houses

offering

Description of  
the appearance,  
course, and  
duration of a  
luminous me-  
teor.

offering a considerable view of the heavens, at that time splendid with stars; I was looking with attention towards the N. W. when suddenly a meteor from about  $35^{\circ}$  of height, shot from the W. by N. It was apparently about the size of a tennis ball, perhaps hardly so large, it was followed by a stream of light which seemed in specks, the length of the train was about a degree, that is about twice the apparent diameter of the moon. Its course was from North of West towards the North, passing about  $10^{\circ}$  below the of the Great Bear, which I judge was then about  $45^{\circ}$  above the horizon. Its motion was majestic, by no means rapid, I am sure it was full ten seconds in motion, the light not so piercing as that of a star of the first magnitude, but exceeded that of the second, with which I had full opportunity of comparing it. It ran through  $30^{\circ}$  of the heavens, describing an arch of great diameter, its path was convex above, and declining downwards. The extinction of it was at an altitude of about  $25^{\circ}$  having fallen certainly not more than  $10^{\circ}$ , I do not think so much. It very visibly stopped before it was extinguished. It burst at last with very few sparks, and its train and itself together disappeared in a moment. I had perfect leisure and space to observe its whole course, it expired below the second pointer, I instantly drew out my watch, and comparing it this morning with the clock of St. Paul's Cathedral, it was exactly at thirty-one minutes after eleven that I observed the end of the phenomenon.

I am, Sir,

Your most obedient humble Servant,

A CONSTANT READER.

Monday, July 22d, 1805.

## IX.

*Precipitation of Platina as a Covering or Defence to polished Steel, and also to Bras. In a Letter from Mr. J. STODART.*

To Mr. NICHOLSON.

DEAR SIR,

Platina is taken  
from its solution  
by ether.

YOU kindly favoured me, by inserting in the last Number of your excellent Journal, an account of a method I have



used with success, for gilding polished steel with gold; perhaps it may be worth knowing, that a very similar process may be performed with platina. That metal, in a state of solution, is taken up from the acid by agitation with ether, in the way that gold is, though certainly with less avidity. The ethereal solution of platina afforded by this process, is, like that of gold, deposited on the surface of polished iron, or steel, <sup>and coats iron or steel;</sup> forming a coat of defence from rust. It is perhaps a fact of equal importance, that the surface of polished brass is coated with platina by the same operation that steel is; namely, by <sup>and also brass,</sup> plunging the brass for an instant into the ethereal solution. As far as I know, these facts have not hitherto been noticed: on the contrary, authors highly respectable, have from ingenious and well conducted experiments, been led to conclusions very opposite to those I have advanced. Dr. Lewis, to whose <sup>Dr. Lewis's experiments gave a contrary effect;</sup> genius and industry the arts are much indebted, says, "gold is the only one of the known metals which the ether takes from acids; and hence this fluid affords a ready method of distinguishing gold, contained in acid solutions." The same author gives the following experiment. "Sulphuric ether was poured into a solution of platina, and into a composition of platina and gold. The vials were stopp'd and shaken, the ether received no colour from the solution of platina, but became instantly yellow from that of the platina and gold." The only way in which I can account for these results, so contrary to my experience, is by supposing that the platina with which Dr. Lewis made his experiments, was not so pure as that with which we are now furnished. What I used was <sup>probably, because his platina was impure.</sup> part of an excellent malleable bar, its specific gravity I do not exactly know. I am inclined to think it was quite pure. The ether was furnished by my friend Mr. Hume, whom I am again happy to thank for his kind and able assistance. The ethereal solution of platina is of a beautiful pale yellow colour, does not at all stain the hand, and is precipitated by volatile alkali. The precipitate I have not examined. It may be fulminating, and I have no relish for explosions. The coat of platina on steel is of a dull white colour. I have no doubt <sup>The platina coating is less beautiful than gold.</sup> of its proving quite as good a defence from rust, as the coat of gold. It is, however, by no means so beautiful; for which reason a preference will probably be given to the last named metal. I have used both the gold and platina in coating different

different parts of the same instrument. The effect produced by the contrast of colour is very beautiful. Whether any of these observations may be worth communicating through the medium of your most useful Journal, is a question I beg leave to submit entirely to your judgment. I have not tried any of the essential oils with solution of platina; further experiments will probably be made with these metallic solutions, by those who have more time, and a better knowledge of these subjects. Such pursuits, when the results are frankly communicated, promise to benefit science, and must ultimately prove useful to society.

I remain with much respect,

Dear Sir, your obliged Servant,

J. STODART.

Strand, July 24, 1805.

## X.

*Experiments on Wootz. By Mr. DAVID MUSHETT. From the Philosophical Transactions, 1805.*

(Concluded from Page 204.)

### Forging No. 2.

Appearances on  
forging the cake  
of wootz, No. 2.

ONE half of this cake was heated to a scarlet shade, and put under the cutting chissel; it was at first struck lightly, then reheated, and cut comparatively soft; but a small crack had over-run the progress of the chissel. Its softness in cutting was attributed to an evident want of solidity. The other half cake felt harder under the hammer, but proved afterwards spongy throughout the mass. In the act of cutting, a loose pulverised matter was disengaged from some of the cells, possessed of a shining appearance.

The fractures obtained in consequence of the division of the half cakes, presented a flattish crystallized appearance, more resembling very white cast iron, than steel capable of being extended under the hammer. One of the middle cuts was entirely cellular with crystallized interiors, and incapable of drawing; the corresponding cut of the other half cake was

drawn

drawn into a strait bar three quarters of an inch in breadth, and three-eighths thick, but was covered with cracks and flaws from end to end. The colour of the break was one shade lighter than No. 1; it tore less out, was equally yolký, and possessed on the whole an aspect very unfavourable for good steel.

The other two outside quarters were also drawn into shape, one under the tilt hammer, and the other by hand. These were more solid in the fracture, possessed fewer surface-cracks, stood a higher degree of heat, tore out more, and exhibited a silky glossy grain, at least two shades lighter in the colour than the centre pieces.

### *Forging 3d Cake.*

One half of this cake, first subjected to be cut, was found softer than any of the preceding, and exhibited no symptom of cracking. The other half was cut at three heats, but found loose and hollow in the extreme. A considerable

Appearances on  
forging the cake  
of wootz, No. 3.

portion of the same brilliant powder, formerly noticed, was here again disengaged. It was carefully taken up for examination, and found to be very fine ore of iron in a pulverescient state, very obedient to the magnet, and without any doubt an unmetallized portion of that from which wootz is made.

This curious circumstance led me to examine every pore and cell throughout the whole fragments. On the upper surface of two of them I found small pits containing a portion of the ore, which had been slightly agglutinated in the fire, but still highly magnetic. The upper surface of the present cake, close by the gate or feeder, contained a large pit filled with a stratum of semi-fused ore, surmounted by a mass of vitrified matter, which bore evident marks of containing calcareous earth.

Those who have devoted sufficient attention to the affinities of iron and earths for carbon, will be surprised to find that, on this particular subject, the rude fabricators of steel in Hindostan have got the start of our more polished countrymen in the manufacture of steel.

Two bars of wootz were formed from this cake, and these in point of quality inferior to any of those formerly produced. The appearance of the metal was more varied, less homogeneous, and contained more distinct laminæ with rusty surfaces, than either of the two former cakes.



It appeared highly probable, from the observations that occurred in forging, and in the examination of the cake, that the original proportion of mixture was such as would have formed a quality of steel softer than No. 1 and 2; but as steel of such softness requires a greater heat to fuse it, than when more fully saturated with carbonaceous matter, it is probable that the furnace had not been sufficiently powerful to occasion complete fusion of the whole mass, and generate a steel homogeneous in all its parts.

#### *Forging 4th Cake.*

Appearances on  
forging the cake  
of wootz, No. 4.

Both halves of this cake cut pleasantly, and with a degree of tenacity and resistance, mixed at the same time with softness beyond what was experienced in any of the former cakes.

Two quarters of this cake were drawn under the tilt hammer, and one by hand. The resulting bars were nearly perfect. A slight scale was observable upon the bar, from that quarter which contained the figure. The fracture was solid, though not homogeneous as to quality and colour, and it appeared pretty evident, that a considerable portion of one side through the whole bar was in the state of malleable iron, and of course not capable of being hardened. It was a subject of considerable regret, that the cake the most perfect and the most tenacious of the whole, in the process of forging, should get an imperfection which rendered it useless for the perfect purposes of steel.

#### *Forging 5th Cake.*

Appearances on  
forging the cake  
of wootz, No. 5.

The first half of this cake cut uncommonly soft for wootz, but by cracking before the chissel still exhibited a want of proper tenacity. The next half cut equally soft, but with more tenacity. Two quarters of this cake drew readily out under the tilt hammer, and a third was drawn by hand at a bright red, sometimes approaching to a faint white heat. None of the bars thus obtained were uniformly free from cracks and scale, although the fracture exhibited a fair break of a light blue colour, and the grain was distinctly marked, and free from yolks.

#### *General Remarks.*

Remarks.  
Wootz appears  
to be the pro-  
duct of a pecu-

The formation of wootz appears to me to be in consequence of the fusion of a peculiar ore, perhaps calcareous, or rendered highly

highly so by mixture of calcareous earth along with a portion of carbonaceous matter. That this is performed in a clay or other vessel or crucible, is equally presumable, in which the separated metal is allowed to cool; hence the crystallization that occupies the pits and cells found in and upon the under or rounded surface of the wootz cakes.

The want of homogeneity, and of real solidity in almost every cake of wootz, appears to me to be a direct consequence of the want of heat sufficiently powerful to effect a perfect reduction; what strengthens this supposition much is, that those cakes that are the hardest, *i. e.* that contain the greatest quantity of carbonaceous matter, and of course form the most fusible steel, are always the most solid and homogenous. On the contrary, those cakes, into which the cutting chissel most easily finds its way, are in general cellular, replete with laminæ, and abound in veins of malleable iron.

It is probable, had the native Hindostan the means of rendering his cast steel as fluid as water, it would have occurred to him to have run it into moulds, and by this means have acquired an article uniform in its quality, and convenient for those purposes to which it is applied.

The hammering, which is evident around the feeder and upon the upper surface in general, may thus be accounted for. When the cake is taken from the pot or crucible, the feeder will most probably be slightly elevated, and the top of the cake partially covered with small masses of ore and steel iron, which the paucity of the heat had left either imperfectly separated or unfused. These most probably, to make the product more marketable, are cut off at a second heating, and the whole surface hammered smooth.

I have observed the same facts and similar appearances in operations of a like nature, and can account satisfactorily for it as follows.

The first portions of metal, that are separated in experiments of this nature, contain the largest share of the whole carbon introduced into the mixture. It follows of course, that an inferior degree of heat will maintain this portion of metal in a state of fluidity, but that a much higher temperature is requisite to reduce the particles of metal, thus for a season robbed of their carbon, and bring them into contact with the portion

liar ore fused,  
and suffered to  
cool in a clay  
vessel;

by an heat not  
sufficient for  
good fusion;

which is the  
reason why it  
was not cast in  
moulds.



portion first rendered fluid, to receive their proportion of the steely principle. Where the heat is languid, the descent of the last portions of iron is sluggish, the mass below begins to lose its fluidity, while its disposition for giving out carbon is reduced by the gradual addition of more iron. An accumulation takes place of metallic masses of various diameters, rising up for half an inch or more into the glass that covers the metal; these are neatly welded and inserted into each other, and diminish in diameter as they go up. The length, or even the existence of this feeder or excrescence, depends upon the heat in general, and upon its temperature at different periods of the same process. If there has been sufficient heat, the surface will be convex and uniformly crystalline; but if the heat has been urged, after the feeder has been formed and an affinity established between it and the steelified mass below, it will only partially disappear in the latter, and the head or part of the upper end of the feeder will be found suspended in the glass that covers the steel.

The same or similar phenomena take place in separating crude iron from its ores, when highly carbonated, and difficult, from an excess of carbon, of being fused.

The division of the wootz cake by the manufacturers of Hindostan, I apprehend is merely to facilitate its subsequent application to the purposes of the artist; it may serve at the same time as a test of the quality of the steel.

Experiments to ascertain the comparative measure of carbon in wootz, by the quantity of lead it reduces from flint glass.

To ascertain, by direct experiment, whether wootz owed its hardness to an extra quantity of carbon, the following experiments were performed with various portions of wootz of common cast-steel, and of white crude iron, premising that in operations with iron and its ores, I have always found the comparative measure of carbon best ascertained by the quantity of lead which was reduced from flint glass.

	1st Cake.	Grains.
1. Fragments of wootz	—	65
2. Pounded flint glass three times the weight	—	195
This mixture was exposed to a heat of 160° Wedgwood, and the wootz fused into a well crystallized spherule of steel. A thin crust of revived lead was found below the wootz, which weighed 9 grains, or $\frac{1}{13\frac{1}{2}}$ the weight of wootz.		

2d Cake.



*2d Cake.*

Grains.

Fragments of wootz - - - - - 80

Flint glass, same proportion as above - - - - - 240

The fusion of the mixture in this experiment was productive of a mass of lead weighing 10 grains, equal to  $\frac{1}{3}$ th the weight of the wootz.

Experiment to ascertain the comparative measure of carbon on wootz, by the quantity of lead it reduces from flint glass.

*3d Cake.*

Fragments of wootz - - - - - 75

Flint glass - - - - - 225

The mass of lead precipitated beneath the steel in this experiment, amounted to 9 grains, or  $\frac{12}{1000}$  the weight of the wootz employed.

*4th Cake.*

Fragments of wootz - - - - - 93

Flint glass - - - - - 279

Lead obtained, precipitated from the glass by means of the carbon of the wootz  $14\frac{1}{2}$  grains, equal to  $\frac{156}{1000}$  the weight of the wootz.

*5th Cake.*

Fragments of wootz - - - - - 69

Flint glass - - - - - 207

The lead revived in this experiment amounted to 7 grains, which is equal to  $\frac{102}{1000}$  the weight of the wootz.

*6th. Cast Steel formed with  $\frac{1}{80}$ th part of its Weight of Carbon.*

Fragments - - - - - 90

Crystal glass - - - - - 270

Lead revived  $8\frac{1}{2}$  grains equal to  $\frac{24}{1000}$  the weight of the steel introduced.

*7th. White cast Iron dropt while Fluid into Water.*

Fragments - - - - - 103

Crystal glass - - - - - 309

The fusion of this precipitated  $23\frac{1}{2}$  grains of lead which is equal to  $\frac{228}{1000}$  the weight of the cast iron.

*Recapitulation of these Experiments.*

Recapitulation of the experiments.	1st cake of wootz revived of lead	,139
	2d ditto	,125
	3d ditto	,120
	4th ditto	,156
	5th ditto	,102
	Steel containing $\frac{1}{66}$ of its weight of carbon	,094
	Cast iron	,228

Wootz contains more carbon than steel does, and less than cast iron.

It would appear to result from these experiments, that wootz contains a greater proportion of carbonaceous matter, than the common qualities of cast steel in this country, and that some particular cakes approach considerably to the nature of cast iron. This circumstance, added to the imperfect fusion which generally occurs in the formation of wootz, appear to me to be quite sufficient to account for its refractory nature, and unhomogeneous texture.

Its ore is probably very excellent.

Notwithstanding the many imperfections with which wootz is loaded, it certainly possesses the radical principles of good steel, and impresses us with a high opinion of the ore from which it is formed.

The possession of this ore for the fabrication of steel and bar iron, might to this country be an object of the highest importance. At present it is a subject of regret, that such a source of wealth cannot be annexed to its capital and talent. Were such an event practicable, then our East India Company might, in their own dominions, supply their stores with a valuable article, and at a much inferior price to any they send from this country.

## XI.

*A Memoir on the Webs of Spiders. By C. L. CADET.\**

On the natural and medical history of spiders.

SPIDERS have often excited the curiosity of naturalists and the attention of physicians. The former have successfully studied the habits and conduct of these insects; and notwithstanding the repugnance they naturally inspire, these accounts

\* Abridged from the *Journal de Physique*, LVIII. 463.

have become interesting, from the industry with which they extend their webs for seizing their prey, and from observations on the multiplicity and arrangement of their eyes, which are geometrically disposed on a motionless head, in a manner conformable to their necessities. Their combats, the singularity of their amours, their sensibility for music, and their patience, all constitute subjects of wonder in the history of spiders. Physicians have examined whether their bite be really venomous, as is generally thought; and they have found only two species productive of danger, namely, the tarantula and avicularia of Cayenne. Swanmerdam, Rossi, and Baglivi have left us little to wish for in this matter, as the effects of their bite and the remedies are both known.

The webs of spiders are considered by the common people as a remedy for wounds; country people often apply them on cuts or slight wounds, and apparently with success. This property was not of sufficient importance to induce chemists to analyse the material; but as there has also been attributed to them a febrifuge virtue, superior in some circumstances to the bark, I have thought them entitled to a more particular examination. The following extract is taken from the *Journal d'Economie Rurale*, for Germinal, in the year XII.

Spiders' webs a popular remedy for slight wounds.

“ We have seen upwards of thirty years ago, a good prior, the curate of Batheren in Franche Comté, cure all the fevers of his parish, and of the neighbouring villages, by pills of a strange composition. He went into his barn and formed small pills with spiders' webs, by rolling them between his hands in the state he found them. He administered this remedy to his patients in white wine, and very seldom failed to cure. M. Marie de St. Urfin being chief physician of the Hotel de Dieu, of Chartres, treated a very obstinate fever in that hospital. He had employed bitters, the bark, and all the remedies of medical art without success, when one of the female attendants offered to undertake the case with a certainty of cure. When she was interrogated concerning her remedy, she refused to mention it. M. de St. Urfin therefore continued to attend his patient for some days; after which, having a good opinion of the attendant, he determined to put his patient under her care. There was no return of the fever after the first dose of the remedy. The physician supposed that the imagination of the patient, his confidence in a new remedy,

Narrative of fevers cured by spiders internally taken.



and particularly the secrecy, might have suspended the attack, and he waited, but to no purpose, for its return. The attendant encouraged by her success, consented to mention the remedy, which proved to be the same as that of the curate of Batheren."

Supposed to be  
effected by gela-  
tine.

The editor of the Journal here quoted, being struck with the new experiments of Seguin upon gelatine applied to the treatment of intermitting fevers, suspects that spiders' webs may contain a principle resembling animal jelly. The experiments of Cadet, while they overthrow this supposition, appear to him entitled to the attention of medical men.

Analysis of spi-  
ders' webs.

*Experiment 1.* Spiders' webs triturated in the cold with quick-lime, emit a slight ammoniacal smell. 2. Cold water by digestion on the webs, becomes of a red-brown colour; is slightly precipitated by infusion of nut-galls; is precipitated by acids; and this precipitate is again dissolved when the acids are saturated with ammonia. 3. Spiders' webs cleaned as much as possible from dust and foreign matters, were boiled in distilled water. The decoction smelled like champignons, and lathered by agitation. The undissolved matter was boiled in additional waters, until it gave out nothing more. All these waters being put together and evaporated, let fall their contents in successive pellicles; and at length, by gentle evaporation, a solid extract was had, nearly equal to half the weight of the spiders' webs. 4. The residue not dissolved in boiling water, was digested in alcohol. It gave a very deep orange-coloured tincture, which did not lather. Water being added, threw down a grey flaky precipitate, of a brown colour when dry, and little more than one hundred and seven-tieth part of the original webs. On hot coals it swelled up, smoked, and took fire; and from its habitudes in these respects, and with the alkalies, it resembled a resin. The diluted alcoholic solution being then evaporated, afforded a residue slightly deliquescent, of a taste at first sweetish, and afterwards bitter, and in quantity nearly three times that of the resinous precipitate. 5. The insoluble residue after this treatment with water and alcohol, burned without swelling up, and emitted a small quantity of white fumes having the smell of burned wood. Neither the oxygenated muriatic, nor the sulphureous acids, discoloured it. It was soluble with effervescence in muriatic acid, which took up two-thirds and

left

left a black paste. Ammonia separated a brown matter in small quantity from the clear solution; and this matter, when calcined, did not lose its colour. It was almost totally soluble in muriatic acid, and this solution gave a black precipitate with nut-galls, and a blue with alkaline prussiate. The fluid to which the ammonia had been added, gave a grey precipitate by pot-ash. This retained its colour when ignited, and was again soluble in muriatic acid with effervescence. 6. Caustic pot-ash poured on the residue of spiders' webs previously treated with water and alcohol, disengages a little ammonia, and partly dissolves the matter. An acid throws down from this solution a black pulvulent tasteless precipitate, which slightly puffs up by heat, and leaves by desiccation a brittle and apparently resinous matter. Its quantity is about one-twelfth of the exhausted matter made use of. It is partly soluble in volatile oils.

7. The aqueous extract of No. 3 being digested with alcohol, gave out one-seventh part. This alcoholic extract was brown, considerably deliquescent, and of a sharp taste. It swelled considerably on the coals, and at a certain period it burned rapidly, as if a nitrate were present. It effervesced briskly with sulphuric acid, giving out a white vapour of a muriatic smell. Potash and lime disengaged from this extract a strong ammoniacal smell, and the vapours were very sensible on the approach of muriatic acid. The extract having been incinerated, appeared by several experiments to contain muriate of lime and a sulphate. What remained of the aqueous extract after treatment with alcohol, was less deep in colour than before, had a purvulent appearance, and slightly pungent taste. On hot coals it did not swell up, but left a very abundant precipitate. Strong sulphuric acid poured on this extract produced no sensible smell, and there was no production of ammonia when it was triturated with quick-lime.

8. Spiders' webs subjected to destructive distillation, gave first water slightly coloured, but becoming deeper as the process went on; and afterwards a black thick oil, with carbonated hydrogen and carbonic acid. A very sensible smell of ammonia was developed, and a residual coal was left, amounting to half the matter employed. The coal after incineration left two-thirds of its weight, half of which was taken up by muriatic acid, and the remainder seemed to be siliceous matter.



Analysis of spiders' webs.

matter. The muriatic solution, during evaporation, deposited sulphate of lime. When spiders' webs were incinerated in an open vessel, the ashes were found to contain sulphate of lime; muriate of soda, and carbonate of soda. Muriatic acid applied to the residue took up more sulphate of lime; and when this solution was treated with ammonia and afterwards with pot-ash, it gave oxide of iron, a little alumine, and some lime. The undissolved part was silex.

9. Spiders' webs were almost totally dissolved in nitric acid amounting to six times their weight; carbonic acid and nitrous gas being disengaged. The solution when evaporated let fall crystals of sulphate of lime, and by continuing the evaporation, the yellow, bitter, deliquescent matter, which Welter calls *qmer*, was afforded.

Component parts.

Hence the author concludes that spiders' webs are composed of, 1. A brown extract soluble in water, and not changeable in the air; 2. A resinous extract soluble in alcohol, and very deliquescent; 3. A small quantity of alumine; 4. Sulphate of lime; 5. Carbonate of soda; 6. Muriate of soda; 7. Carbonate of lime; 8. Iron; 9. Silex. The author thinks that the earths and earthy salts may be derived from the local situation of these insects, and that it is probable that the webs of garden spiders may not afford them. The two constant products to which he demands particular attention, are those obtained from the aqueous and alcoholic solutions. He thinks it desirable to try their medical powers separately. He supposes the resinous matter to be the same substance as under other circumstances forms the spiders' silk, and the wax which Mr. Accum has elsewhere mentioned as one of their products.

## XII.

*Information on the Mines and Manufactures of the East Indies, and other Subjects. By J. MACHLACHLAN, Esq. of Calcutta.\**

SIR,

Account of some receipts for dying.

SHOULD you think the enclosed receipts for dying the beautiful reds of the Coromandel coast can be of any use to

\* See Arts, 1804; for which the silver medal was given.

the



the dyers of the united British kingdom, be pleased to lay them before the Society for the Encouragement of Arts, &c. that they may be published in the volume of their Transactions; if not, I trust you will excuse my troubling you with them. They were sent to me from Madras by a scientific friend, who had the several operations, detailed in them, performed in his own presence. I forwarded a copy of them, and a small quantity of the ingredients mentioned in them, to a friend at home, several years ago; but he dying about or soon after the time of their arrival, I never learned what became of them. It strikes me, however, that there is a considerable coincidence between the thread process and that which I have seen recommended by Mr. Henry, of Manchester, for dying the Adrianople or Turkey red.

I am not certain whether it is known at home, that many of the hills in Bahar, and other parts of India, contain immense quantities of mica, talc, or Muscovy glass. The natives of this country and China make very splendid lanterns, shades, and ornaments of it, tinged of various fanciful colours; and it is also used by them in medicine. When burned or calcined, it is, I am told, considered as a specific in obstinate coughs and consumptions. When powdered, it serves to silver the Indian paper, &c. used in letter-writing; and, in fact, it is applied to numberless purposes. The bazar price of that of the best quality, split into sheets of about two lines thick, is six rupees the maund of 84lb. avoirdupois. If it could be applied to any useful purpose at home, it might go in part ballast of ships, and at a trifling expense. I enclose a small specimen of it, and am,

Immenſe quantities of talc found in the hills in Bahar.

Sir,

Your very obedient servant,

J. MACHLACHLAN.

Calcutta, Oct. 4, 1803.

N. B. The chaya, or red dye root of the Coast, is, I believe, known at home: as also the cashaw leaves, which are used as an astringent.

CHARLES TAYLOR, Esq.

Directions

*Directions for dying a bright Red, four Yards of  $\frac{3}{4}$  broad Cotton Cloth.*

Instructions for  
dying cottons  
bright red by  
the Indian me-  
thod.

1st. The cloth is to be well washed and dried, for the purpose of clearing it of lime and congee, or starch, generally used in India for bleaching and dressing cloths; then put into an earthen vessel, containing twelve ounces of chaya or red dye root, with a gallon of water, and allow it to boil a short time over the fire.

2d. The cloth being taken out, washed in clean water, and dried in the sun, is again put into a pot with one ounce of myrabolans, or galls coarsely powdered, and a gallon of clear water, and allowed to boil to one half: when cool, add to the mixture a quarter of a pint of buffalo's milk. The cloth being fully soaked in this, take it out, and dry it in the sun.

3d. Wash the cloth again in clear cold water, and dry it in the sun; then immerse it into a gallon of water, a quarter of a pint of buffalo's milk, and a quarter of an ounce of the powdered galls. Soak well in this mixture, and dry in the sun. The cloth, at this stage of the process, feeling rough and hard, is to be rolled up and beetled till it becomes soft.

4th. Infuse into six quarts of cold water, six ounces of red wood shavings, and allow it to remain so two days. On the third day boil it down to two-thirds the quantity, when the liquor will appear of a good bright red colour. To every quart of this, before it cools, add a quarter of an ounce of powdered alum; soak in it your cloth twice over, drying it between each time in the shade.

5th. After three days wash in clean water, and half dry in the sun; then immerse the cloth into five gallons of water, at about the temperature of 120 degrees of Fahrenheit, adding 50 ounces of powdered chaya, and allowing the whole to boil for three hours; take the pot off the fire, but let the cloth remain in it until the liquor is perfectly cool; then wring it gently, and hang it up in the sun to dry.

6th. Mix intimately together, by hand, about a pint measure of fresh sheep's dung, with a gallon of cold water, in which soak the cloth thoroughly, and immediately take it out, and dry it in the sun.

7th.

7th. Wash the cloth well in clean water, and spread it out in the sun on a sand-bank (which in India is universally preferred to a grass-plat) for six hours, sprinkling it from time to time, as it dries, with clean water, for the purpose of finishing and perfecting the colour, which will be of a very fine bright red.

Instructions for  
dying cottons  
bright red by  
the Indian me-  
thod.

J. MACHLACHLAN.

Calcutta, Oct. 4, 1803.

CHARLES TAYLOR, Esq.

*Directions for dying of a beautiful red, eight ounces of Cotton Thread.*

1st. Put one gallon and a half, by measure, of sap-wood ashes, into an earthen pot, with three gallons of water, and allow the mixture to remain twenty-four hours to perfect it for use.

2. Put the following articles into an earthen pot, viz. Three-quarters of a pint of Gingelly oil; one pint, by measure, of sheep's dung, intimately mixed by hand in water; two pints of the above ley.—After mixing these ingredients well, pour the mixture gradually upon the thread into another vessel, wetting it only as the thread, by being squeezed and rolled about by the hand, imbibes it, continuing to do so until the whole is completely soaked up, and allow the thread to remain in this state until next day.

3d. Take it up, and put it in the sun to dry; then take a pint and a half of ash-ley, in which squeeze and roll the thread well and allow it to remain till next day.

4th. Squeeze and roll it in a like quantity of ash-ley, and put it in the sun to dry; when dry, squeeze and roll it again in the ley, and allow it to remain till next day.

5th. Let the same process be repeated three or four times, and intermit till next day.

6th. Ley the thread once, as the day before, and, when well dried in the sun, prepare the following liquor: One gill of Gingelly oil; one pint and a half of ash-ley.—In this squeeze and roll the thread well, and leave it so till next day.

7th. Repeat the process of yesterday, and dry the thread in the sun.



Instructions for  
dyeing cottons  
bright red by  
the Indian me-  
thod.

8th. The same process to be repeated.

9th. First repeat the ash-ley process three or four times, as under the operations 3, 4, and 5, and then prepare the following mixture: On pint of sheep-dung water; one gill of Gingelly oil; one pint and a half of ash-ley.—In this squeeze and roll the thread well, and dry it in the sun.

10th. Repeat the same process.

11th. Do. Do.

12th. Do. Do.

13th. Do. Do.

14th. Do. Do.

15th. Wash the thread in clean water, and squeeze and roll it in a cloth until almost dry; then put it into a vessel containing a gill of powdered chaya root, one pint by measure of cashan leaves, and ten pints of clear water; in this liquor squeeze and roll it about well, and allow it to remain so till next day.

16th. Wring the thread, and dry it in the sun, and repeat again the whole of the 15th process. leaving the thread to steep.

17th. Wring it well, dry it in the sun, and repeat the same process as the day before.

18th. Do. Do.

19th. Do. Do.

20. Wring and dry it in the sun, and with the like quantity of chaya root in ten pints of water, boil the thread for three hours, and allow it to remain in the infusion until cold.

21st. Wash the thread well in clear water, dry it in the sun, and the whole process is complete.

J. MACLACHLAN:

Calcutta, Oct. 4, 1803.

## SCIENTIFIC NEWS.

*Imperial Academy of Sciences at Petersburg.*

THE Vice Admiral Tchitchagoff, Minister of the Marine department, has forwarded to the Academy a question on the resistance of fluids, and its application to naval architecture, for the solution of which that department will bestow a reward of 1000 ducats of Holland, or 462*l.* 10*s.*

Prize of the  
Russian Imperial  
Academy.

The academy being desirous of seconding the patriotic views of the marine department, decided on the publication of a program in the following terms.

Prize proposed by the department of the Marine, on the 1st of July, 1804.

It is proposed, that of the two theories of the resistance of fluids proposed and applied to naval architecture by Don G. Juan, in his *Examen Maritime*, and by M. Romme, in his *Art de la Marine*, one or the other of them, for example, that of Don Juan should be corrected and improved to such a degree, as to afford results that shall differ from the results of experiment, by so small a quantity, as may be practically neglected without sensible error:—Or otherwise, if these theories cannot be corrected, it is proposed, that a new theory should be established and applied to naval architecture, which shall lead to conclusions of the same degree of accuracy;—Or otherwise, lastly, if it should be impossible to establish such a theory, it is proposed, that from experiments at least, there should be deduced a formula resembling those which have been given by Messrs. Bossut and Prony; and such that it shall be not only more conformable to experiments than those formulas, but that it shall lead as nearly as possible to the conclusions drawn from experiments, even when the formule shall be applied to naval architecture.

The subject.  
To improve the  
theories of the  
resistance of  
fluids;

or to establish a  
new theory;

or to deduce a  
formula from  
experiment.

For the satisfactory solution of this problem the department of the marine has appointed a prize of 1000 Ducats of Holland, and has fixed a term of two years to be reckoned from the date of this program. After the expiration of that term, no memoirs addressed to the academy will be received on this subject, the time appointed being sufficient for those new experiments which the solutions in question render indispensable.

One thousand  
ducats, or 462*l.*  
10*s.*

bly necessary. The memoirs forwarded to the academy must be written in a distinct legible character, either in the French English or Russian language.

(Signed in the original) PAUL TCHITCHAGOFF.

Terms and conditions for receiving memoirs.

In the invitation accompanying this program, the Academy requires men of science who intend to make application for this prize, to address their memoirs to the perpetual secretary of that body, before the 1st of July, 1806, and that the writer should clear the post charges as far as the regulations of their respective countries will allow. The customary mode of marking the memoirs with a device or motto, and sending at the same time a sealed letter, having the same device, and containing the name and residence of the author, is also to be adopted in the present instance. The memoirs will be examined before the expiration of the term of concurrence, by the Department of the Marine and by the Academy; the latter of whom will publish the judgment they shall adopt, and the Department of the Marine will crown by the payment of the prize, the labours of that author who shall have satisfied the conditions of the program.

*Prize concerning Light, proposed by the Imperial Academy of Sciences for the Year 1806.*

Prize concerning light.

The usual theories that light is projected matter; or mere undulation of a fluid;—

—or it is a chemical principle.

Five hundred roubles, £.112 10 0

The question.

After an introduction, in which a concise statement is given of two theories respecting the nature of light, the one ascribed to Newton, which supposes light to consist in the emanations of matter from luminous objects, and the other ascribed to Euler, which deduces the effects from vibrations of a peculiar elastic fluid,—they proceed to state rather more fully a chemical hypothesis of Lavoisier, who not only considers light as caused by a peculiar matter, but also that this matter is subject to the elective attractions, so as by its combinations and disengagements to produce an extensive series of phenomena, which are thus accounted for. It is principally with a view to developpe this last hypothesis that they have proposed a prize of 500 roubles (£112 10 0.)

“For the most instructive series of new experiments on light, considered as matter; on the properties which may with justice be ascribed to it; on the affinities which it shall appear to have with other bodies, whether organic or inorganic, and

on



on the modifications and phenomena which are manifested in those substances by virtue of the combinations in which the matter of light may have entered along with them."

After proposing this question, the Academy proceeds to explain, by observing that without entering into any historic discussion, or the objections which have been opposed to this hypothesis, nor the researches already made with a view to develop traces of chemical action between light and bodies in the different modifications of natural phenomena,—the enquiries here proposed may not be unusefully extended to the galvanic fire, of which the dazzling brilliancy when large piles are made to act upon coally matters, in some respect imitates the solar light. The Academy has chosen to enunciate the subject of their prize in a general way, in order that philosophers may not be in any respect impeded as to the points of view from which they may be disposed to contemplate and to treat so difficult a subject, which has scarcely yet been entered upon, though so eminently worthy of attention from the cultivators of natural science.

The memoirs are to be written either in Russian, French, English, German, or Latin, and forwarded to the perpetual secretary of the Academy, sealed up, with device and indicative billet, as mentioned with regard to the former prize. No memoirs will be received after the 30th of April, 1806, inclusive, and the author of that memoir which in the judgment of the Academy shall have merited the prize, shall be proclaimed in the public meeting of the following month of July. The successful memoir becomes the property of the Academy, and must not be printed without their formal permission. The other treatises will be delivered to the respective authors, on application to the secretary, either personally or by procuration.

#### *Voyages of Messrs. HUMBOLDT and BONPLAND.*

Messrs. Levrault, Schoell and Company circulated at the beginning of the present year a prospectus of the voyage of Messrs. Humboldt and Bonpland, the publication of which is committed to them: "the travellers, they observe, have in general re-written all their observations, whatever might have been the object, in the narrative of their voyage. Mr. Humboldt has thought it proper to follow another course, and to

treat

Remarks by the  
academy.

Conditions and  
terms.

Voyages of  
Humboldt and  
Bonpland.

Humboldt's  
travels.

treat separately all those objects which considerably differ in their nature. He has determined accordingly to publish in detached collections, all that more particularly belongs to Astronomy, Geology, Botany, Zoology, &c. and his voyage, properly so called, will embrace all that relates to General Physics, the Origin of Nations, their Manners, their Intellectual Culture, Antiquities, Commerce, and Political Economy. Upon this part of his observations and the history of his voyage, he will not at present publish more than a narrative, under the title of *Relation abrégé*, &c. or an abridged Relation of a Voyage to the Tropics, performed in the interior of the new continent during the years 1799, 1800, 1801, 1802, and 1803.

It is agreed by Messrs. Humboldt and Bonpland, who are connected by the most intimate sentiments of friendship, and have shared together in all the fatigues and dangers of this voyage, that the whole of their publications shall be in their joint names. The preface to each work will announce to which of them the several parts are respectively to be ascribed. The list of works speedily to appear are as follows:

1. The abridged Relation of the Voyage, in quarto; promised in July, 1805.

2. A Collection of Astronomical Observations and Admeasurements made on the new continent; promised in 1805.

3. An Essay on the Geography of Plants, or a Philosophical Sketch of the Equinoctial Regions; founded on observations made from the 18th degree of south latitude, in the years before mentioned, with one large plate, coloured; promised in June, 1805.

4. Equinoctial Plants, collected in Mexico and the Isle of Cuba, in the Provinces of Caracas, Cumena and Barcelona, on the Andes of New Grenada, Quito and Peru, and on the banks of Rio Negro, Oroonoko and the River of Amazons, with plates engraved by *Sellier*, in folio; the first number to appear in April, 1805.

5. A Collection of Observations of Zoology and comparative Anatomy, made in a Voyage to the Tropics; with plates engraved by *Bouquet*, coloured or not, at the option of the purchaser. The first number to appear in May.

N.B. All these works will bear the general title of *Voyage de M. M. Alexandre de Humboldt et Aimé Bonpland*, and will form

form a collection of the same size and type, except the Equinoctial Plants, which are larger. Subsequent notices of the price and publication are to appear in the journals.

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*Fish ejected from Volcanoes.*

AMONG the great number of facts which Humboldt has collected in his voyage, the following lately communicated to the National Institute is very curious. Several volcanoes of the Cordilleras of the Andes occasionally throw out eruptions of mud mixed with large volumes of fresh water, and what is most remarkable, an infinite number of fishes. The volcano of Imbaburo, among others, threw out at one time so great a number near the town of Ibarra, that their putrefaction occasioned disorders. This phenomenon, astonishing as it appears, is not even extraordinary, but, on the contrary, of considerable frequency, so that the facts are authentically preserved in the public registers, along with those of earthquakes. It is more particularly singular that these fish are not at all injured, though their structure is very soft. They do not even appear to have been exposed to a high temperature; for the Indians assert that they sometimes arrive at the foot of the mountain still living. These animals are sometimes thrown out of the mouths of the crater and sometimes through lateral clefts; but always at the height of 12 or 1300 toises or fathoms above the surrounding plains. Humboldt thinks they are produced in lakes situated at that height within the crater; and it is a confirmation of this opinion that the same species are found in the brooks which run at the foot of the mountains. It is the only species which subsists at the height of 1400 toises. It is a new species to naturalists. Humboldt made a drawing of it on the spot, and gave it the name of *Pimelodrus Cyclopus* or *thrown by the Cyclops*. It will be found in the first number of his Zoology.

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*Water formed by Mechanical Pressure.*

IN a sitting of the French National Institute at the commencement of the present year, M. Biot read a note on the formation of water by mere compression. The experiment of forming water out of its component parts oxygen and hydrogen by burning those gases by the electric spark is well known. M. Biot has succeeded in determining this combination independently of electricity, by rapidly compressing a mixture of the

Volcanic eruption of fishes.

Combustion of oxygen and hydrogen by mechanical pressure.

the



the two gases included in the syringe of an air-gun. The compression which forces the particles of gas together, causes them to give out a sufficient quantity of heat to set them on fire. Some caution is requisite in repeating this experiment, which is not without danger. Out of three times that M. Biot made it, there were two in which the brass cap of the pump and the barrel itself, which was iron, were broken by the explosion.

### *Malleability of Zinc.*

Zinc is malleable while heated between 210° and 300°.

A VERY curious and useful discovery has been made by Messrs. Charles Hobson and Charles Sylvester, both of Sheffield, that zinc is in fact a malleable metal. The laminability of this metal to a certain considerable degree, has long been known; but it was not suspected that it is capable of being forged and drawn into wire. They have found that at a temperature between 210° and 300° of Fahrenheit zinc yields to the hammer, and also that it may be wire-drawn or laminated by keeping it at this temperature during the mechanical operation. An oven or a hollow metallic vessel kept at a due heat may be used for the pieces, in the same manner as the Smith's forge is used for iron and steel. It appears that the zinc, after having been thus annealed and wrought, continues soft, flexible and extensible, and does not return to its former partial brittleness, but may be bended and applied to the uses for which zinc has hitherto been thought unfit, such as the fabrication of vessels, the sheathing of ships, and numerous other important applications. I have seen a chafed or stamped figure raised at one stroke in thin zinc; which is, I think, as much elevated as it could have been in copper.\*

### *Palladium.*

Palladium.

BY a letter from Messrs. Knight, of Foster-lane, I am informed that the new metal, palladium, may be purchased at their warehouse.

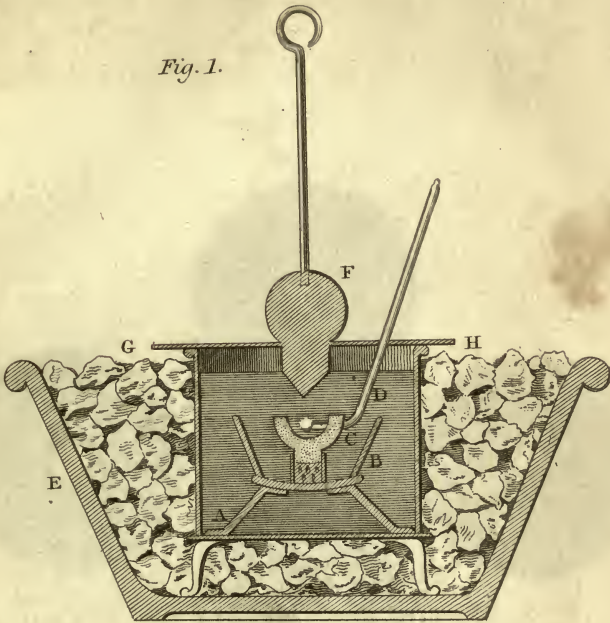
Erratum corrected.

MR. GREGORY begs leave to correct a mistake which occurs in his paper on horse-powers, occasioned by his inadvertently copying from some of his former observations on that subject the fraction  $\frac{2}{3}$  instead of  $\frac{2}{5}$  for the approximate value of the exponent  $n$ : this makes a change in the results of the computation at page 148; for  $(9-3)^{\frac{2}{3}} : (9-4.4)^{\frac{2}{3}} :: 130 : 80.58$ , or nearly  $80\frac{1}{2}$  lb. instead of  $71\frac{1}{2}$ , as there given. He also points out a press error at p. 153, where the symbols of multiplication in each of the theorems should have been signs of addition.

\* The inventors have obtained a patent.

*New Experiments by Count Rumford to prove the maximum of Density in Water above 32° Fahr.*

Fig. 1.



*Chevalier Edelcrantz's Valves for Steam Engines.*

Fig. 2.

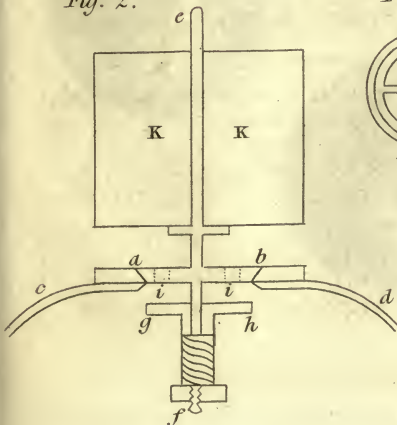
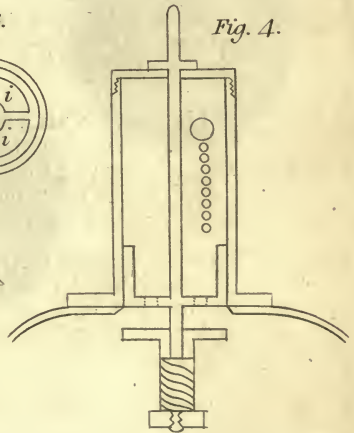
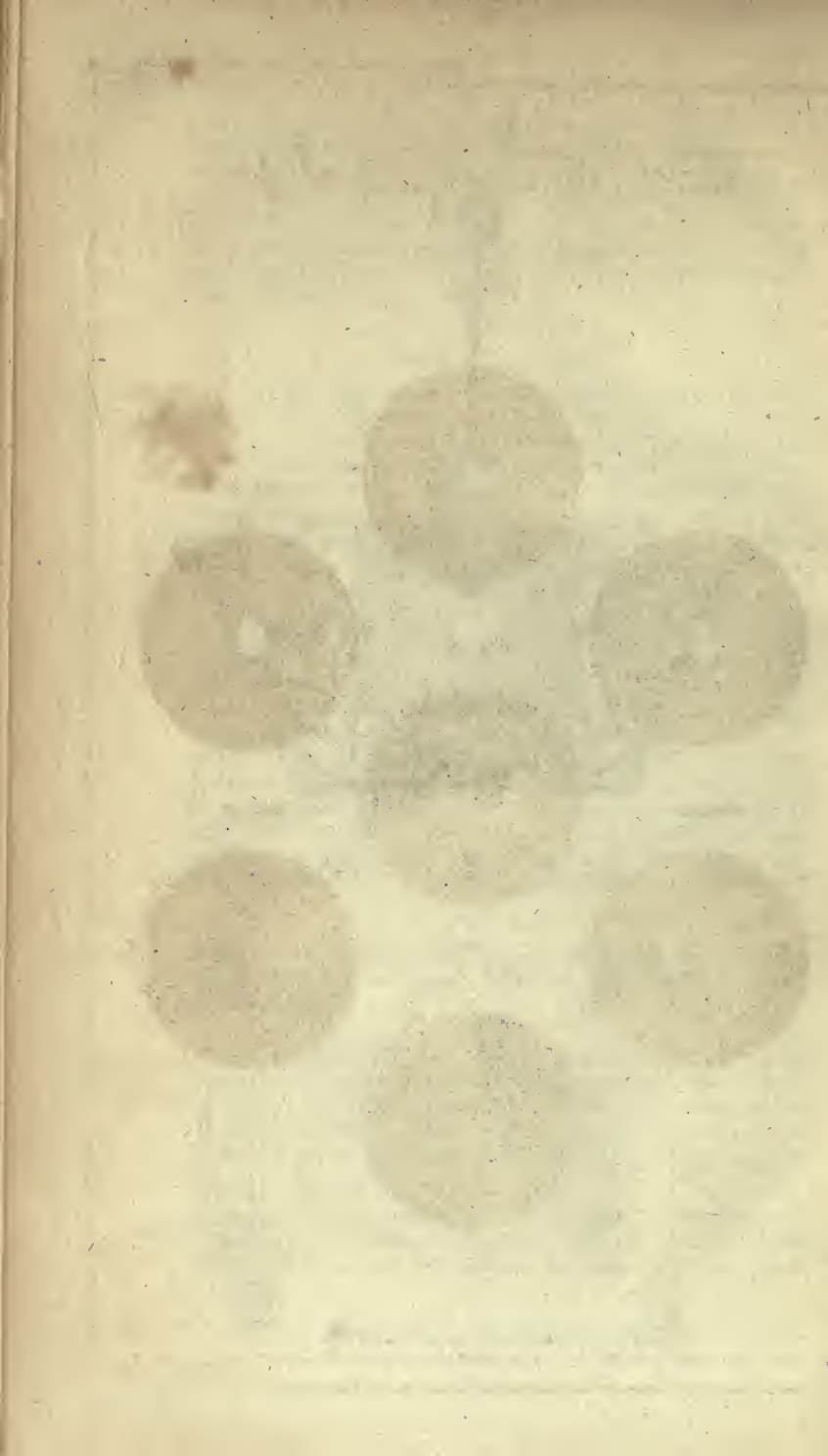


Fig. 3.



Fig. 4.





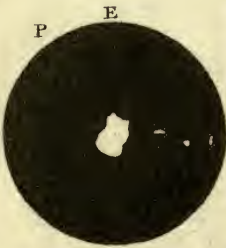


*Variable Star in Sobieski's Shield.*

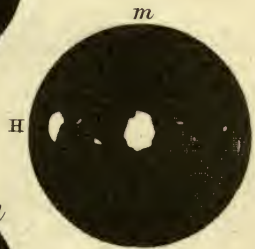
*Fig. 1.*



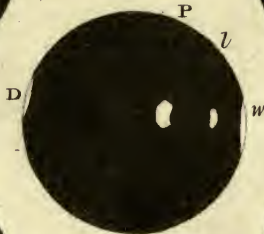
*Fig. 2.*



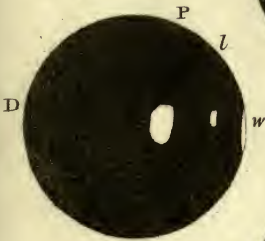
*Fig. 3.*



*Fig. 4.*



*Fig. 5.*

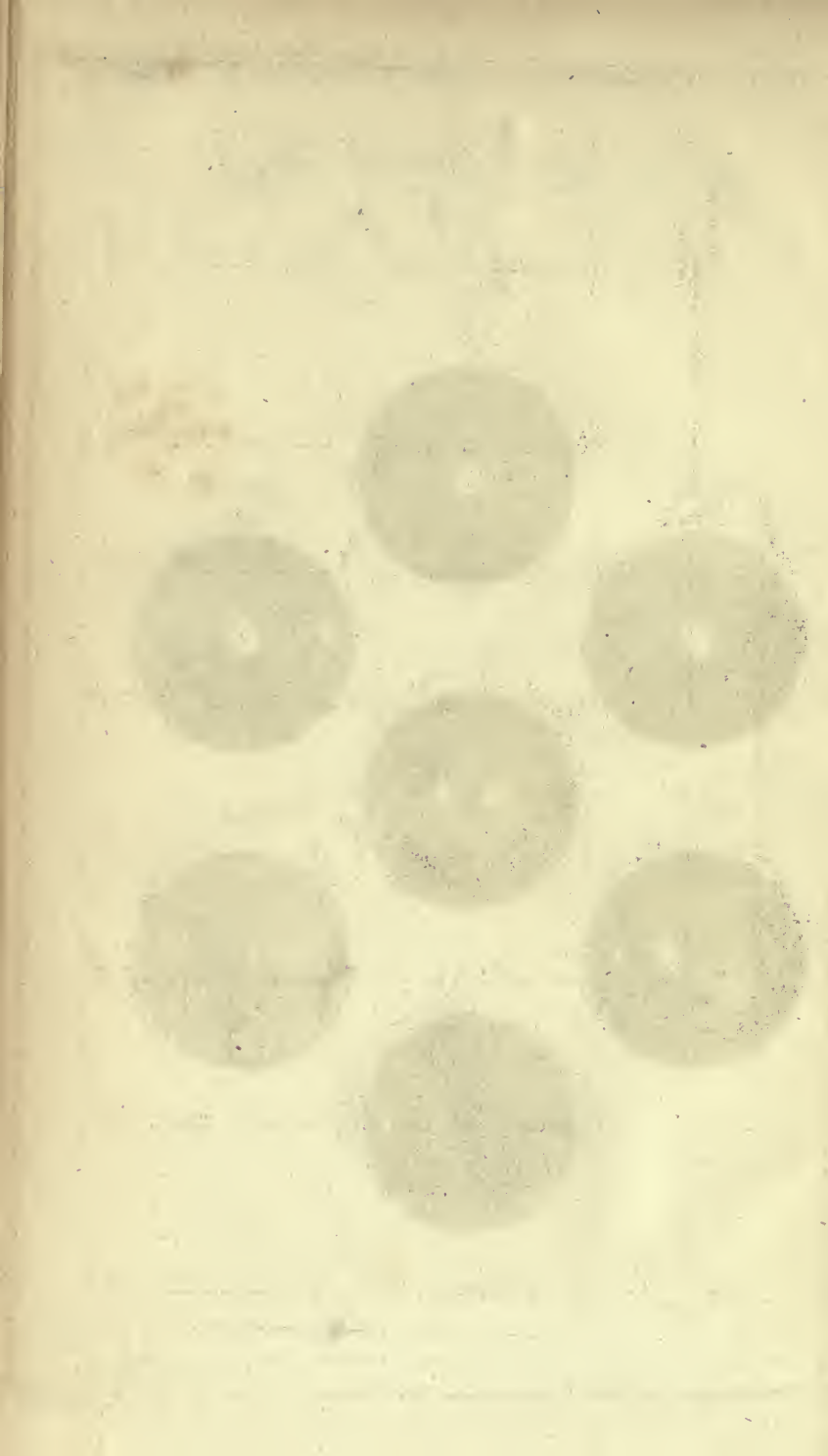


*Fig. 6.*



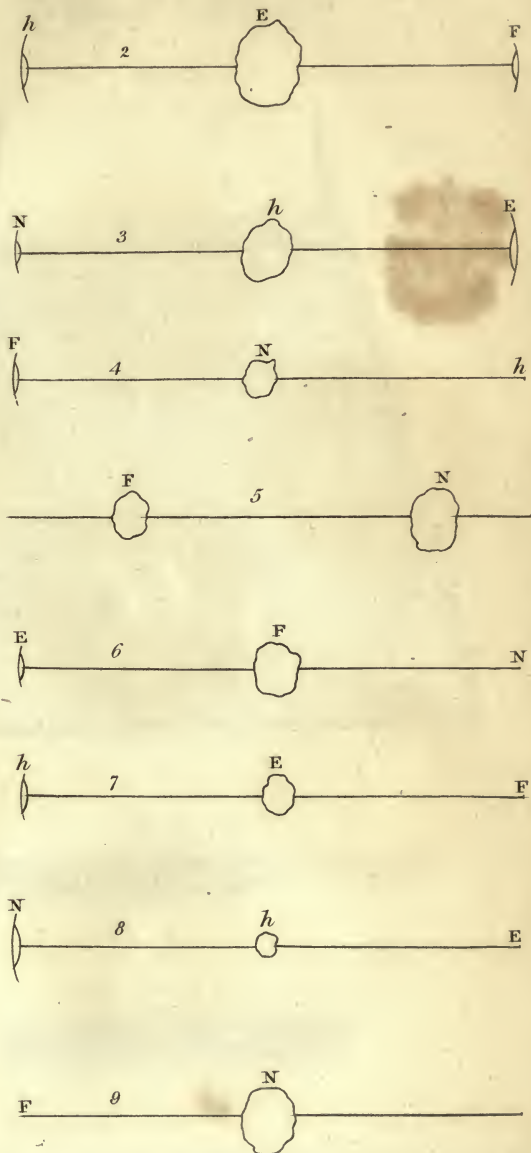
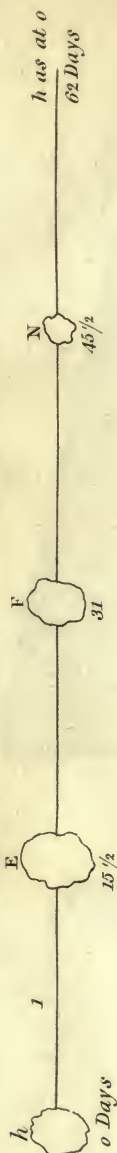
*Fig. 7.*





# Spherical Views.

An extended View of the Surrounding Spots June 18<sup>th</sup> 1796.







Mr. Harrison's Press for  
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Fig. 1.

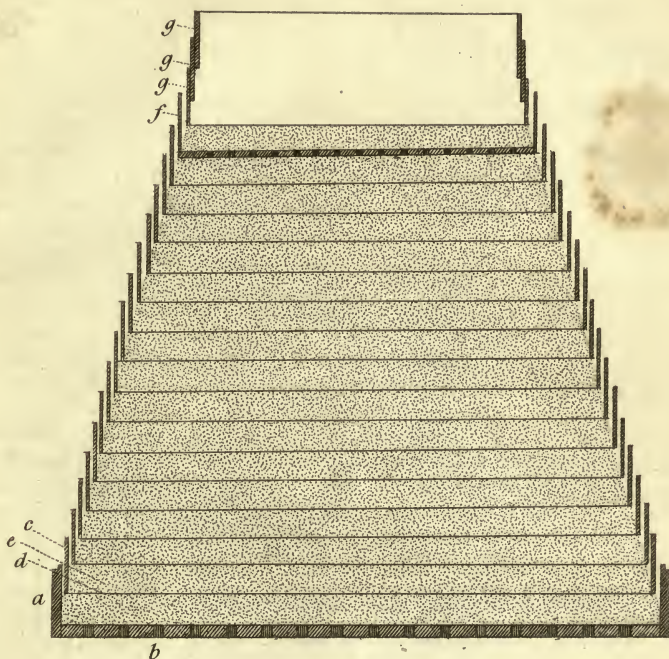
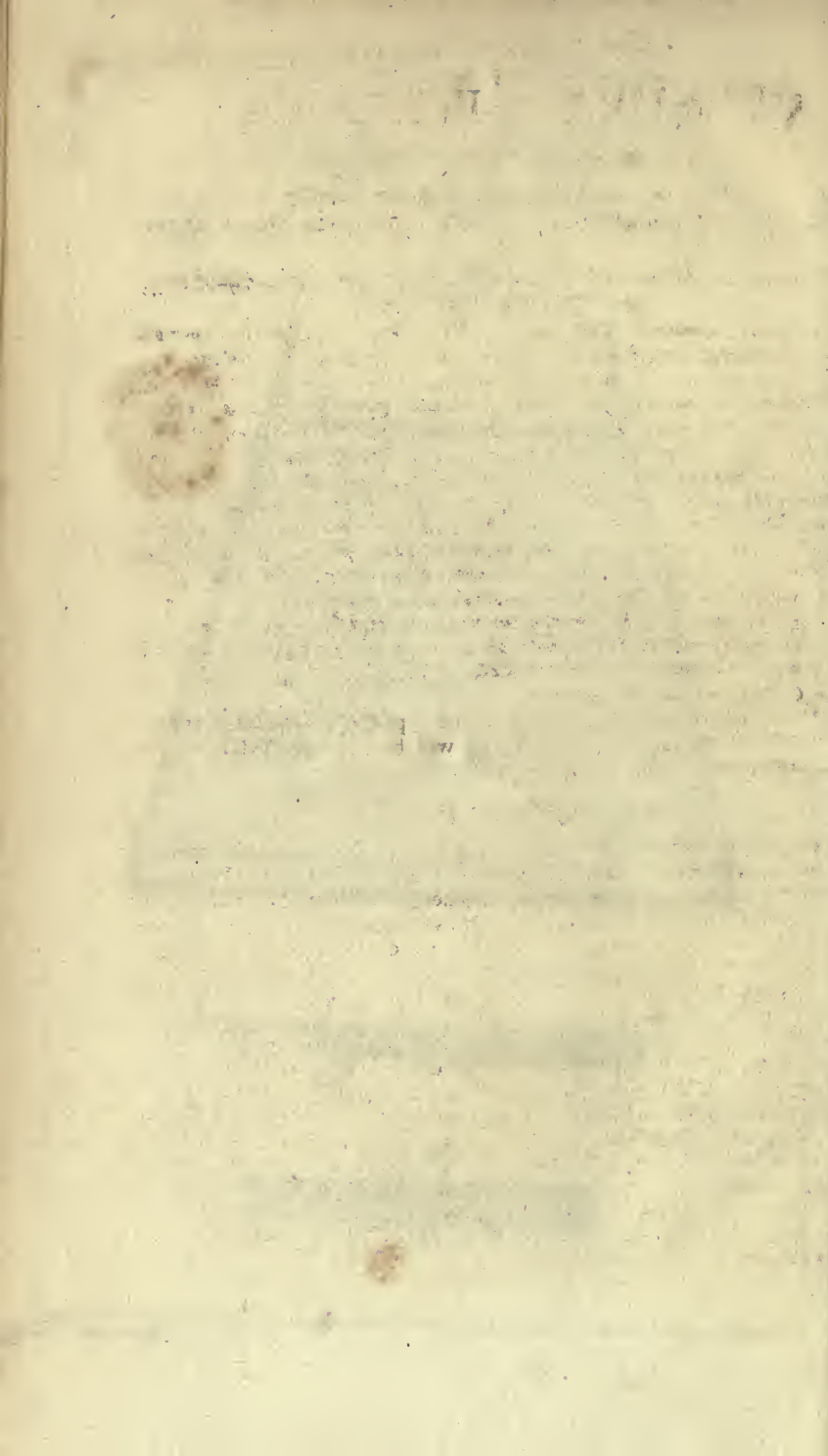


Fig. 2.



Fig. 3







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